Essays on the Microeconomics of Development in Tanzania

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Dedication

To the parents and children of the Meatu District in Tanzania, who graciously shared their concerns, desires, and time, and made all of this work possible.

To the community-based distributors of Meatu District Hospital, whose passion for economic development inspires mine. *Nashakuru.*

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Chapter 1

Introduction

In the eighteenth and nineteenth centuries, the work of Jeremy Bentham and John Stewart Mill reflected an early understanding of economics as utilitarianism: "It is the greatest happiness of the greatest number that is the measure of right and wrong." (Bentham, 1996). The writing of Bentham and Mill on utilitarianism informed the modern-day model of microeconomics and serves as the inspiration for utility maximization and social welfare functions. Both the standard microeconomic model of household decision-making under scarcity and the notion of Pareto efficiency have their roots in utilitarianism. Mill's theory of individual worth and self-improvement, as the source of true freedom, influences the thinking on development economists today. Amartya Sen, who conceived the capabilities approach to development, describes the development process as one of expanding freedom and capabilities for individuals to lead the kind of life they value. In this early classical period of the economics, development economics was at the center of the field of economics.

In this dissertation, I build from the foundation of intellectual history in the field of microeconomics of development to understand problems of poverty in today's developing world. At the time of Bentham and Mill, poverty and industrialization made England the developing country of study, whereas today, I write about Tanzania.

As of 2015, Tanzania was the twelfth largest economy in Africa, yet 68 percent of

the population lives on less than $1.25 per day (World Bank, 2014). Despite important economic reforms and a six percent growth rate in the last decade, poverty remains widespread across the country. The Human Development Index (HDI), which is an aggregate index of life expectancy, education, and income, places Tanzania in the bottom quartile of all countries in the world in terms of total human development. Sixty three percent of households have no access to piped water for drinking, and 67 percent of households live in dwellings with a floor made of earth or sand (United Nations Development Program, 2014). Life expectancy was 61 years in 2012, which reflects a ten year increase since 2002. While 80 percent of primary school-age children were attending school in 2012, chronic child malnutrition is estimated to be an underlying cause of over one third of child deaths (under age 5).

Following the independence of Tanganyika in 1961 and Zanzibar in 1963, the two regions merged in April 1964 to form the United Republic of Tanzania. Although unified under the national language of Swahili, Tanzania's diverse population comprises over 100 different tribal groups and languages. A high total fertility rate (5.1 births per woman) and decreasing infant mortality rate have contributed to a rapidly growing population. The total population may triple by 2050 if the current natality trends continue (United Nations Population Division, 2015). This growth has also contributed to a near doubling of the labor force between 2001 and 2012. Most of this labor force is in the agricultural sector: in 2012, 80 percent of Tanzanians were working in agriculture.

This dissertation contributes to a growing body of research on the microeconomics of development in Sub-Saharan Africa. Fertility, labor market participation and agriculture are key components of the microeconomic development process in Tanzania. I explore household and individual decisions in all three of these domains in Tanzania through microeconomic analysis and impact evaluation. Both experimental and non-experimental impact evaluations improve the public understanding of what works in economic development.

This dissertation is organized as follows. For the first essay in Chapter 2, I explore household fertility decisions by estimating the effect of a community family planning education program on fertility behavior in the Meatu District. In Chapter 3, I investigate

the effects of an entrepreneurship training program on financial literacy and employment attitudes in the Kagera region. This essay is joint with Brooke Krause and David Chapman. In Chapter 4, I analyze the impact of polygyny on agricultural productivity in farming households across the country. This essay is joint with Amy Damon and Vincent Siegerink. Finally, Chapter 5 concludes.

Chapter 2

His and Her Fertility Preferences: An Experimental Evaluation of Asymmetric Information in Family Planning

2.1 Introduction

When men desire nearly three times as many additional children as their wives and possess most of the decision-making power in the household, the discordance of preferences leads to excess fertility and welfare losses for wives, who bear almost all of the costs of pregnancy and child-rearing. High rates of fertility persist in sub-Saharan Africa, where, in 2013, the total fertility rate was 5.1 births per woman, relative to the total fertility rate of 2.3 births per woman in the rest of the world (World Bank, 2014). The rate in rural Tanzania stood even higher, at 8.4 births per woman (DHS, 2010). In fact, if the current natality trends in Tanzania continue, its population will triple by 2050 (United Nations Population Division, 2015). The benefits of planned and spaced births include positive outcomes for children, including better nutrition and more years

of schooling (Do and Phung, 2010), and better maternal health (Winikoff, 1983; Norton, 2005). While previous economic studies indicate that high fertility is usually a consequence of large desired family sizes (Rosenzweig and Wolpin, 1980a,b; Moffitt, 2005), it is not yet clear how high fertility is affected by heterogeneous spousal fertility desires.

According to the 2010 Tanzania Demographic and Health Survey (DHS), 27 percent of rural Tanzanian women say that they would like to delay a birth by at least two years but are not using contraception. This gap is even larger in the data collected for this study. In the 2012 baseline household data from the Meatu district of northern Tanzania, 76 percent of women report that they want to delay a birth by at least two years but are not using contraception. Knowledge of fertility control is poor in this context. Eighty percent of women believe that folkloric methods of birth control (such as luck charms) are effective in preventing pregnancy. Additionally, husbands generally have more pronatalist fertility preferences than their wives in Sub-Saharan Africa (Ezeh, Seroussi, and Raggers, 1996). This preference is also confirmed in the Meatu household survey, where women, on average, report desiring an additional 1.4 children and men report desiring an additional 4.5 children. The lack of knowledge about family planning methods combined with heterogeneous fertility preferences among spouses may prevent women from achieving their desired family sizes.

This study addresses the problem of wives' excess fertility by proposing two main research questions. First, can the number of unwanted pregnancies be reduced through an informational family planning program that reduces the psychosocial cost of contraceptives? And secondly, in the presence of heterogeneous spousal fertility preferences, what is the effect of including husbands in family planning consultations? I measure the impact of family planning worker household consultations through a small field experiment that randomized the inclusion of husbands. The study sample includes approximately 600 randomly-selected households across 12 villages in the district of Meatu. I use a conceptual framework based on non-cooperative game theory to explain fertility decisions and make predictions about the effect of family planning information on fertility behavior under different expectations about husbands' violent behavior.

The main findings provide evidence of a trade-off between welfare gains for women

and marital gains from better-aligned preferences. The family planning program reduced psychosocial cost of contraception adoption and thus reduced pregnancies significantly in the treatment group. Women who consulted with the family planning worker individually (without their husbands) had a significantly larger reduction in pregnancies than women who consulted together with their husbands. Yet, the joint conversation about family planning as part of the couples consultation also had an effect, reducing men's (relatively large) fertility desires. And, in contrast to predictions by the conceptual framework, when women expect their husbands to be abusive, the asymmetric family planning information (excluding husbands from consultations) has a negative effect on pregnancies.

This study builds on a body of literature on the determinants of fertility choices and intra-household bargaining. Service delivery through the decentralized provision of sexual and reproductive health care using locally-based health workers has proven effective in rural areas of developing countries. The seminal experimental Matlab Project in Bangladesh showed that through a community health worker program, poor populations reduced fertility rates and improved child health (Bhatia et al., 1980). Several studies have documented the sizable impact of this particularly intensive program, and showed that family planning efforts can affect fertility even in the absence of major socioeconomic improvements (Bhatia et al., 1980; Joshi and Schultz, 2007; Sinha, 2005).

However, observational studies of changes in fertility in developing countries lack random assignment of family planning policies or programs. When program placement is not exogenous to the outcome, a number of unobservable factors (e.g. demand for contraceptives, labor market, status of women) may lead to biased estimates of the program impact (Pitt, Rosenzweig, and Gibbons, 1993; Molyneaux, 1994). This evaluation challenge is particularly problematic amid economic development and rising levels of income (Pritchett, 1994; Miller, 2009). Pritchett (1994) argued that the supply of family planning services is not a dominant determinant of differing fertility rates because fertility is largely determined by demand. And rising income and economic development affect the main determinants of couples fertility desires: the relative costs of children versus other goods, the couple's income, and their preferences for children versus competing forms of consumption (Becker, 1960). I overcome the evaluation challenge by

implementing a randomized field experiment. Although the region may see rising incomes over the study period, the information provided in household family planning consultations was randomly assigned to villages.

The role of husbands' preferences in intra-couple fertility decisions has been evaluated through experimental designs that exploit random inclusion of men in family planning consultations. Terefe and Larson (1993) first examined the experimental effect of men in family planning decisions in urban Ethiopia and discovered that women who consulted with a family planning nurse while their husbands were present were more likely to adopt contraceptive methods than women who consulted with the nurse alone. Ashraf, Field, and Lee (2014), however, found contrasting evidence about the role of husbands in Zambia. They administered a one-time voucher for access to discrete contraceptives through household family planning consultations. The authors found that women who received the voucher privately (without their husbands) were more likely to seek family planning services than women who received the voucher with their husbands. The distinction in Ashraf, Field, and Lee (2014) provides evidence of women taking advantage of asymmetric information to behave strategically and achieve their own desired fertility.

My contribution is three-fold. First, because the psychosocial cost of concealed contraceptive use is borne over time, I expand on Ashraf, Field, and Lee (2014) by examining intra-household bargaining over fertility for a longer-term (fifteen-month) family planning intervention, allowing more time for spousal discussion, in a region where women have limited intra-household bargaining power (as empirically supported by minimal decision-making ability within the household). Second, for the unresolved question on whether husbands should be included in family planning education, my results provide evidence of the positive cooperation effects of inclusion while also supporting private welfare gains for women in individual consultations. And finally, I provide evidence that women who experience intimate-partner violence are more likely to seek out family planning services through the intervention and to reduce pregnancies.

This chapter is organized as follows. Section 2.2 outlines the conceptual framework for understanding spousal behavior. Section 2.3 presents the methods of implementation

of the randomized field experiment. Section 2.4 discusses the empirical strategy for measuring the program impact. Section 2.5 presents descriptive statistics. Section 2.4 presents and discusses the empirical results, and Section 2.7 concludes.

2.2 Conceptual Framework

In this section, I develop a framework that describes inter-spousal family planning decisions to make predictions about behavior that I test in the empirical analysis. The basic model is similar to the non-cooperative framework used in Ashraf, Field, and Lee (2014), although I simplify the model payoffs in order to explicitly solve for best response functions and then examine the changing effect of husbands' behavior. This model predicts two key testable hypotheses: (1) a reduction in the psychosocial cost of contraception adoption leads to an increase in the use of contraceptives (with a corresponding reduction in pregnancies); and (2) whether women adopt contraceptives depends on their expectations of their husbands' violent behavior.

2.2.1 Non-cooperation and Inefficiency

The collective model of the household describes two agents making decisions that affect one another (Manser and Brown, 1980; McElroy and Horney, 1981). The weights on agents' utility functions are thought to be affected by external factors such as income. Through bargaining over household resources, the couple reaches decisions that are Pareto efficient. The consequences of intra-household bargaining have been empirically observed in fertility decisions, household finances and investments in children (Thomas, 1990; Duflo, 2000; Rangel, 2006) However, a key assumption for efficiency in collective bargaining is mutual knowledge of each others' preferences, resources and choices, which includes perfect information and perfect contracts between spouses. According to baseline Meatu data, most couples (65 percent) have never had any conversation about fertility desires or family planning, so it is unlikely that the couples have bargained efficiently to the point of reaching a binding agreement. Further, the assumption of

efficiency in collective bargaining has been rejected by empirical evidence, especially in Sub-Saharan Africa (Duflo and Udry, 2004; Udry, 1996).

Rasul (2008) frames a model of collective bargaining over fertility and finds that investments in fertility are efficient only if couples agree to a contract, or binding commitment, on the number of children to have. Despite this, the empirical evidence indicated that all types of couples bargain without commitment. Referencing the hold-up problem, Rasul (2008) concludes that without commitment, the influence of each spouse's fertility preferences depends on the individual's bargaining power within the marriage (Grossman and Hart, 1986; Hart and Moore, 2008).[1] Unequal levels of bargaining power allow for opportunistic behavior when one spouse is exposed to private information. Individuals have been shown to use money and information differently when given the opportunity to hide these resources from their spouse (Castilla and Walker, 2013; Aker, McClelland, and Tierney, 2014). Thus, under the collective model, asymmetric information between spouses is a potential source of inefficient household decisions and inefficient investments in fertility (Ashraf, Field, and Lee, 2014; Kebede et al., 2013).

The evidence of unsuccessful fertility contracting between couples (Rasul, 2008), the potential advantage of private information about contraceptives, and evidence from the Meatu context suggest a non-cooperative fertility bargaining framework with incomplete information. The non-cooperative framework does not assume efficiency at the outset and allows for limited commitment and asymmetric information about resources, choices and preferences. Lundberg and Pollak (1993) provide the original framework for a non-cooperative household model with limited commitment and Chen (2013) expands the model in the case of imperfect information.

In the non-cooperative model without commitment, each person's action is a best response to his or her spouse's actions. I characterize an extensive form game of incomplete information. The husband (H) and wife (W) cannot reach a contract on fertility behavior, so they choose actions that maximize their own payoffs. The players in the

[1] The hold-up problem results when agents refrain from cooperation and do not reach efficient contracts due to unequal levels of bargaining power.

game include Nature, Wife, and Husband. Nature moves first and makes contraception available ($A = 1$) with probability α, or unavailable ($A = 0$).[2] The availability of contraceptives is observed only by the wife. She observes Nature's action and, if contraceptives are available, makes the second decision, choosing to adopt contraception ($C = 1$) with probability κ, or not ($C = 0$). The husband also does not observe this action.[3] If contraceptives are not available, she does not take contraceptives ($A = 0$ implies $C = 0$). If contraceptives are not adopted, Nature moves again in deciding if a birth will take place ($B = 1$) with probability β, or no birth ($B = 0$). If contraceptives are adopted, no birth takes place ($C = 1$ implies $B = 0$). The husband observes this final action of Nature and is allowed the possibility to feel aggrieved in response to a no birth outcome (husbands are assumed to be pronatalist) and thus choose to punish ($P = 1$) with probability π, or not to punish ($P = 0$).[4] In this context, punishments can be understood as intimate partner violence, which is prevalent in this district. In the model, the husband uses the threat of violence in attempt to convince her not to take contraceptives.[5] If a birth occurs, the husband does not punish ($B = 1$ implies $P = 0$).

The players, actions (in capital letters), probabilities (under each node) and payoffs (on the far right) can be viewed in Figure 2.1. Because this is an extensive form game of incomplete information, it is useful to outline what each player knows and does not know. The wife knows the availability of contraceptives (A), whether she has adopted them (C), whether a birth has occurred (B). She does not know whether the husband will punish (P), but she does know the probability that he will punish (π). The husband knows whether or not a birth has occurred. He does not know whether contraceptives are available (A) or whether his wife has taken them (C), but he forms beliefs about the availability (α) and the likelihood that she will take them (κ). He also knows the

[2]Nature is a game theoretical representation of luck. While in reality, availability of contraceptives is determined by health provisions and societal acceptance, these outside factors are simplified and represented by Nature in this framework.

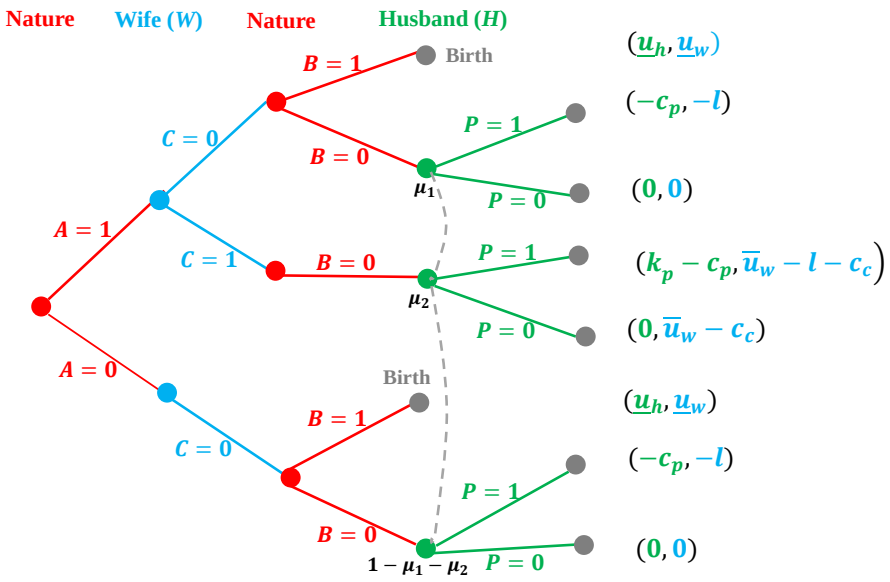
[3]The most popular contraceptive methods in Tanzania are quarterly injections (e.g. Depo Provera), the pill or female sterilization. Because condoms are not popular in this region, the model assumes that women chose to adopt female-centered contraception.

[4]He punishes through a process that Hart (2008) refers to as "shading", in which the husband inflicts negative behavior on his wife as a result of feeling short-changed by the outcomes.

[5]In fact, 35 percent of women in the baseline survey had experienced intimate partner violence and a few women in focus group discussions recalled threats of violence related to contraceptive use.

range of these probabilities ($0 \leq \alpha \leq 1$; $0 \leq \kappa \leq 1$). The wife's choice variable is C and the husband's choice variable is P, which they both determine by maximizing their own expected utilities. The final outcomes are represented by the nodes on the right side of the figure with corresponding payoffs displayed as (H,W).

Figure 2.1: Conceptual Framework Game



The payoffs to W depend on her utility of a giving birth to a child now ($\underline{u}_w$), her utility of delaying the birth of a child ($\overline{u}_w$),[6] the (psycho-social) cost of adopting contraception (c_c), and the utility loss imposed by a punishing husband (l). W is assumed to prefer a delayed birth, $\overline{u}_w > \underline{u}_w$. The highest payoff for W is in the case of taking contraception, delaying a birth and not experiencing punishment. Her lowest payoff is in the case of not taking contraceptives yet also not giving birth. The payoffs to taking contraceptives, however, depend on the probability of punishment, π.

[6]This term captures the utility of a delayed birth for women, but it also represents the confidence that a woman has knowing that she will not get pregnant at this time. Although she may not have a birth when she does not take contraception, she does not gain the utility of $\overline{u}_w$ in those cases because she knows the likelihood of a birth is high and that this is determined by nature (luck).

The payoffs to H depend on the utility of a birth ($\underline{u}_h$), the gain he receives from punishing when she is using contraception (k_p)[7] minus the additional cost he bears of being a punishing husband (c_p). The cost of being a punishing husband is not large enough to lower his utility because the gain from punishing is at least as large as this cost ($k_p \geq c_p$). The highest payoff for H is in the case of a birth, while his lowest payoff is in the case of no birth and imposing punishment.

The payoffs to each player depend on the framework's parameters, but can be ranked from highest to lowest utility.

$$-l < \underline{u}_w < 0 < \overline{u}_w - \pi l - c_c < \overline{u}_w - c_c \text{ for } \pi < X \text{ (wife)} \tag{2.2.1}$$

$$\text{For some value of } X, \text{ such that } 0 \leq X \leq 1 \tag{2.2.2}$$

$$-c_p < 0 < k_p - c_p < \underline{u}_h \text{ (husband)} \tag{2.2.3}$$

2.2.2 Best Response Functions

The husband's expected utility can be defined in terms of parameters and probabilities. For simplicity sake, I use μ_1, μ_2 and $(1 - \mu_1 - \mu_2)$ to represent H's beliefs that he is at each subgame choice set (decision node). The top, middle and bottom decision nodes are informationally equivalent, although his beliefs may vary. In this case, $\mu_1 = \alpha(1-\kappa)(1-\beta)$ ($H's$ top decision node), $\mu_2 = \alpha\kappa(1-\beta)$ ($H's$ middle decision node) and $1-\mu_1-\mu_2=(1-\alpha)(1-\beta)$ ($H's$ bottom decision node). The husband does not observe A or C; he only observes B, whether a birth has occurred. He has beliefs, though, about the probability of available contraceptives and the probability of his wife taking contraceptives. Because the husband does not punish if a child is born, his decision of whether to punish depends only on his expected utility function for the payoffs and probabilities that involve no birth. The game results in the following expected utilities

[7] Although the husband does not directly observe A, and thus cannot know that he is punishing while she is using contraceptives, he can gain utility from k_p based on his own belief that he is correct (which is a function of α and κ).

for the husband to punishing, $P = 1$, and not punishing, $P = 0$:

$$E[U_h^{P=1}] = \mu_1 * (-c_p) + \mu_2 * (k_p - c_p) + (1 - \mu_1 - \mu_2) * (-c_p) \tag{2.2.4}$$

$$E[U_h^{P=0}] = \mu_1 * (0) + \mu_2 * 0 + (1 - \mu_1 - \mu_2) * 0 \tag{2.2.5}$$

To solve for $H's$ best response function and determine the conditions under which he will punish, I first define the indifference surface. Based on μ_1 and μ_2, this surface expresses $H's$ indifference between choosing $P = 1$ or $P = 0$.

$$E[U_h^{P=1}] = E[U_h^{P=0}] \tag{2.2.6}$$

$$\mu_1 * (-c_p) + \mu_2 * (k_p - c_p) + (1 - \mu_1 - \mu_2) * (-c_p) = 0 \tag{2.2.7}$$

$$\mu_2 = \frac{c_p}{k_p}$$

$$\alpha\kappa(1 - \beta) = \frac{c_p}{k_p}$$

The husband is indifferent between choosing $P = 1$ or $P = 0$ when the above condition is true. The husband prefers to punish when: $\alpha\kappa(1-\beta) > \frac{c_p}{k_p}$. Intuitively, this indicates that if the probability of W taking contraceptives when they are available and not having a birth are larger than the cost-benefit ratio of being a difficult husband, then he will punish. The husband is more likely to punish when he believes it is highly likely that his wife will have access to, and desire for, contraception. His best response function σ_h follows.

$$\sigma_h = \begin{cases} \pi = 1, & \mu_2 > \frac{c_p}{k_p} \\ \pi \in [0, 1], & \mu_2 = \frac{c_p}{k_p} \\ \pi = 0, & \mu_2 < \frac{c_p}{k_p} \end{cases}$$

Next, I solve for the wife's best response function to determine the conditions under which she will choose to take contraceptives. I define her indifference surface through expected utility of her actions. The wife's choice between taking contraceptives, $C = 1$,

or not, $C = 0$, results in the following expected utilities:

$$E[U_w^{C=1}] = \pi * (\overline{u}_w - l - c_c) + (1 - \pi) * (\overline{u}_w - c_c) \tag{2.2.8}$$

$$E[U_w^{C=0}] = (1 - \beta) * \pi * (-l) + (1 - \beta) * (1 - \pi) * 0 + \beta \underline{u}_w \tag{2.2.9}$$

Based on the probabilities and payoffs of each choice, her indifference surface can be defined by solving for the conditions that equate the expected utilities:

$$E[U_w^{C=1}] = E[U_w^{C=0}]$$

$$\overline{u}_w - \pi l - c_c = \beta \underline{u}_w - (1 - \beta)\pi l$$

$$c_c = \overline{u}_w - \beta(\underline{u}_w + \pi l) \tag{2.2.10}$$

The wife is indifferent between choosing $C = 1$ or $C = 0$ when the above condition is true. She will choose contraceptives when $c_c < \overline{u}_w - \beta(\underline{u}_w + \pi l)$. In other words, she will take contraception when the cost of adopting is not too high. Her best response function, $\sigma(w)$, is written more formally as:

$$\sigma_w = \begin{cases} \kappa = 1, & c_c < \overline{u}_w - \beta(\underline{u}_w + \pi l) \\ \kappa \in [0, 1], & c_c = \overline{u}_w - \beta(\underline{u}_w + \pi l) \\ \kappa = 0, & c_c > \overline{u}_w - \beta(\underline{u}_w + \pi l) \end{cases}$$

Testable Hypothesis 1

An important observation here is that as the psychosocial cost of contraception (c_c) decreases, the woman is more likely to adopt contraception. This testable hypothesis predicts how the first research question will be answered. In the experimental context, although contraceptives are free, the psychosocial cost of adopting contraceptives (e.g. acquiring health information and defying social stigma) may be preventing women from acheiving desired fertility. This psychosocial cost is lowered through the family planning

intervention, as health information is brought to individuals in their home and conversations with a trusted community member reduce the social stigma of contraceptives. I test whether women in the treatment groups are more likely to adopt contraceptives and reduce pregnancies than women in the control group.

2.2.3 Characterization of Equilibria

Here I will characterize the Bayesian perfect equilibria of this game (three pure strategies and a mixing strategy). I define each equilibrium as a pair of the players' actions, $[H, W]$ and discuss each possible solution. I begin by conditions for the husband to be indifferent between punishing, $P = 1$, and not punishing, $P = 0$. The husband's indifference surface can be reduced to:

$$\mu_2 = \frac{c_p}{k_p}$$

I first discern the equilibria solutions when the husband is violent. Based on conditions derived in the wife's best response functions, the equilibrium strategy $[P = 1, C = 1]$ is subgame perfect equilibria if:

$$c_c < \overline{u}_w - \beta(\underline{u}_w + l) \tag{2.2.11}$$

In order for the equilibrium strategy $[P = 1, C = 1]$ to be a Bayesian perfect equilibrum, the conditions of their actions must be supported by beliefs. Since this strategy implies he will punish and she will use contraceptives, I apply his belief about μ_2 and infer that:

$$\alpha \geq \frac{c_p}{k_p} \tag{2.2.12}$$

If both 2.2.11 and 2.2.12 hold, then the solution $[P = 1, C = 1]$ is a Bayesian perfect equilibrium. Next, I determine whether $[P = 1, C = 0]$ can be a subgame perfect equilibrium. If the husband knew that the wife was playing $C = 0$, based on his best response function, he would never choose the lower payoffs associated with $P = 1$. Therefore, sequential rationality implies the solution $[P = 1, C = 0]$ cannot be subgame

perfect. Within the second research question on whether husbands should be included in family planning consultations, I explore two testable hypotheses related to expectations on the husbands behavior. When the husband is likely to punish, how will the loss he imposes have an effect on contraceptive use? I explore how her best response may change in the case where he is likely to be a punishing husband by varying π. She has beliefs about the value of π based on prior experiences. I compare $U_w^{C=1}(\pi = 1)$ to $U_w^{C=0}(\pi = 1)$, using equation 2.2.8 and 2.2.9.

$$E[U_w^{C=0}(\pi = 1)] = \overline{u}_w - l - c_c <> \beta\underline{u}_w - (1-\beta)l = E[U_w^{C=0}(\pi = 1)] \tag{2.2.13}$$

$$l > \underline{u}_w + \frac{\overline{u}_w + c_c}{\beta} \tag{2.2.14}$$

Testable Hypothesis 2a

When the husband is likely to punish ($\pi = 1$), the threat of imposing l is effective in inducing the wife to not take contraception. In 2.2.13, we can see that l has a larger negative effect on the left hand side (when taking contraceptives). For any values of c_c and β, a one unit increase in l will reduce W's expected utility by one. On the right hand side, l reduces her utility by $(1-\beta)$, having a relatively less negative effect. So, she would choose $C = 0$. In this case, his threat of abuse is effective in affecting her behavior: l will make the wife choose not to adopt contraception. This is the last testable hypothesis. When women expect husbands to be abusive, they would be less likely to adopt contraceptives and reduce pregnancies.

Next, I give the conditions for equilibria solution when the husband is not violent. Applying sequential rationality to the wife's best response function, the equilibrium strategy $[P = 0, C = 0]$ can be subgame perfect equilibria if:

$$c_c \geq \overline{u}_w - \beta(\underline{u}_w) \tag{2.2.15}$$

This strategy implies that she will not use contraceptives, thus applying Bayes rule of supporting beliefs implies that $\mu_2 = 0$. The players actions remain best responses given the updated beliefs, so $[P = 0, C = 0]$ is a Bayesian perfect equilibrium. Likewise, applying sequential rationality to the wife's best response function, I determine the

conditions for $[P = 0, C = 1]$ to be subgame perfect:

$$c_c \geq \overline{u}_w - \beta(\underline{u}_w) \tag{2.2.16}$$

Adding to this condition, this solution will be a Bayesian perfect equilibrium if the players' actions are supported by their beliefs. In this case, knowing that she is using contraceptives and applying the husband's belief about μ_2 implies that:

$$\alpha \leq \frac{c_p}{k_p} \tag{2.2.17}$$

If 2.2.17 holds, then [P=0, C=1] is a Bayesian perfect equilibrium.[8]

Expanding on the second research question, I explore the conditions necessary for the wife to choose contraceptives given that the husband is expected to never punish. I compare her payoffs to each choice under the H chooses $P = 0$. This is the comparison of $EU_w^{C=1}(\pi = 0)$ to $EU_w^{C=0}(\pi = 0)$, applying $\pi = 0$ to equation 2.2.8 and 2.2.9. Taking contraception will be optimal when:

$$E[U_w^{C=1}(\pi = 0)] = \overline{u}_w - c_c > \beta\underline{u}_w = U_w^{C=0}(\pi = 0) \tag{2.2.18}$$

$$c_c < \overline{u}_w - \beta\underline{u}_w \tag{2.2.19}$$

Testable Hypothesis 2b

Equation 2.2.19 indicates that if the husband is not going to punish his wife, the wife will take contraceptives when the cost of doing so is small relative to a function of her utility of delayed birth less the expected utility of an early birth. Note that this definition of the conditions for a $[P = 0, C = 1]$ equilibrium depend only on her own utility functions and the probability of a birth, not on the loss of punishment. The husband's threats to induce her to avoid contraception will not be effective (will not change her behavior) if she believes the probability of him being a punishing husband is 0. Testing the effect of the treatment under different expectations of his behavior provides insight into whether husbands should be included in family planning consultations. When women do not

[8] The mixing strategy is also a Bayesian perfect equilibrium when the following two conditions hold. (1) $\overline{u}_w - \beta(\underline{u}_w + l) > c_c > \overline{u}_w - \beta\underline{u}_w$ and (2) $\frac{\alpha\kappa}{\alpha\kappa+\alpha(1-\kappa)(1-\beta)+(1-\alpha)(1-\beta)} = \mu_2 = \frac{c_p}{k_p}$.

expect husbands to be abusive, they would be more likely to adopt contraceptives and reduce pregnancies.

2.3 Methods and Procedures

The data for this study come from a household survey of 660 households across 12 villages in Meatu District of northern Tanzania. The sample was drawn in the following manner. Of the 19 wards in Meatu district, 9 were randomly selected to be included in the study. Those 9 wards contain 48 villages, of which 12 were randomly selected to participate in the study. Tanzanian law requires researchers to gain permission from village leaders to conduct research in each village. The village leaders in all the original 12 villages agreed to participate. At the village level, each village officer provided a list of every household residing in the village. These household lists were divided by sub-village (2-8 sub-villages per village); 2 to 5 sub-villages were randomly selected from each village for the study. Within each of the 2-5 selected sub-villages, an equal number of households from each sub-village were randomly selected from the prepared household rosters to be included in the sample. Approximately 5 percent of the households originally selected refused to participate and were replaced. Households were considered eligible for participation in the study if they contained a married woman age 13 to 40 and the woman's husband also was living in the household.[9]

The Meatu household survey was implemented in August-November 2012, before the family planning program began, and again starting in July 2014, after the program ended. Due to attrition and migration, the second round of the household survey was not completed until February 2015. This household survey includes separate questionnaires for men and women, both of which include modules on socioeconomic status, health and family planning, spousal relations and agriculture. An average of 55 households were interviewed in each of the 12 study villages (60 households from ten of the villages; 30

[9]If more than one wife was living in the household, the field staff interviewed the oldest wife who was still under 40 years old. This occurred in approximately 10 percent of households. If multiple pairs of spouses were living in the household and eligible, the couple which included the head of household was interviewed. This occurred in approximately 5 percent of households

households from two villages).

The family planning education program was cluster-randomized at the village level and treatment assignment was stratified along village-level baseline contraceptive use. The family planning program began with a reproductive health training for the community based distributors, provided by the Ministry of Health.[10] Three literate women from each of the eight treatment villages were selected to participate in the training at the district capital, Mwanhuzi, in February 2013. These 24 women then returned to their own villages, where they began work as "community-based distributors" (CBDs), consulting with households about family planning and working with the local dispensary.

Each CBD was paid monthly for visiting households in her village to share the information from the training and to discuss family planning options. During household visits, CBDs were trained to greet all family members first, and then to ensure a private discussion (either for wives or for husbands and wives together). The consultations included a discussion of the benefits of birth spacing, questions to gauge interest in family planning, review of the long-term and short-term methods available and the fact that they are available free of charge, and information about the process of acquiring contraceptives. Because exactly three CBDs per village were selected for the work and paid to visit at least forty households per month, the number of CBD visits per household varies with village size. In general, smaller villages were treated more intensively, with a larger number of household visits, over the fourteen-month intervention. In most cases, the entire village was treated with the CBD visits. But in three of the larger villages (Villages 3, 10 and 11), one to two sub-villages were dropped from the treatment to reduce the amount of work required by the CBDs.[11] The treatment intensity varies from a household visit once every two weeks (mostly in the smaller villages) to a few visits per year. Seventy-three percent of households who were visited by a CBD had

[10] The training curriculum originated from a UNICEF handbook on family planning and child health. The teachers at the training were employed by the district hospital as public health educators, specializing in sexual and reproductive health.

[11] The dropped sub-villages were chosen based on two criteria: 1) They were not part of the random sample of sub-villages during the baseline and 2) They were not where the CBD lived. The CBDs were slightly more likely to live in sub-villages close to the village center. The sub-villages close to village center have more off-farm work opportunities, thus some bias in the sample selection may have been induced.

at least four visits per year (which could mean up to six visits over the course of the intervention given the 4 month duration of the follow-up household survey).[12]

To explore asymmetric information in fertility decisions over the course of the fifteen-month intervention, the treatment villages were split from the outset into two arms. In one treatment group (four villages), the CBDs consulted with the woman alone (individual treatment group), and in the other four villages, the distributors consulted with the couple together (couples treatment group). This split treatment approach allows one to measure the effect of asymmetric information in household decision-making; husbands in the first treatment group did not receive the information about methods and availability of family planning. Households in the four control villages received no consultations. The second research question, about whether to include husbands, will be tested by comparing the two treatment arms. The individual treatment group meets the criteria for the non-cooperative game defined above because the treatment design excludes husbands from information about the availability of contraceptives. The exclusion of the husband reduces his ability to explicitly prohibit contraceptive use, thus allowing her to choose between $C = 1$ and $C = 0$. The two testable hypotheses under this question (if the husband is expected [not] to be abusive, is she less [more] likely to take contraceptives?) will be explored in measuring the effect of the individual treatment. The geographical dispersion of households in the individual treatment, couples treatment and control group can be viewed in Figure 2.2. Each blue dot represents a household in the individual treatment, each black dot represents a household in the couples treatment and each red dot represents a control household.

In many cases, opposition from husbands, parents-in-law or from the women themselves prevented the intervention from being fully implemented. Although CBDs were encouraged to visit every household within their assigned sub-villages or village, if there was a conflict or opposition to their visit to a given household, they would not continue to pursue consultations with that household. The CBDs in each village estimated the approximate percentage of households who turned away the visits, and this estimate

[12]The household survey data do not include information on which CBDs visited each home. And due to the decentralized implementation, it is also possible that any woman or couple was visited by multiple CBDs. For these reasons, in the analysis, it is not possible to control for *which* CBD visited each woman.

ranged from one in four households (Villages 3, 4 and 10) to one in twenty households (Village 2). Despite the fact that the CBDs reported that they visited almost all households, 36 percent of households assigned to the treatment group reported that they did not have any CBD visits. This effect is not substantially different across treatment arms: 32 percent of couples treatment households did not report visits, and 40 percent of individual treatment households did not have visits. The households who turn away the CBD visits can be classified as non-compliers (did not take up the treatment, despite assignment). Compliance varies starkly across villages. In Village 2, 94 percent of households were visited by a CBD. However, in Village 10, where the CBDs were unable to complete assigned work, only 23 percent of households were visited by a CBD.[13]

The map also displays the distribution of village health dispensaries (similar to small clinics with pharmacies). Most women (75 percent) who use contraceptives report that they heard about their current method at the dispensary. As can be seen in Figure 2, many villages have their own dispensary, although in some cases, several villages share a dispensary or clinic (with dispensary). Figure 2 distinguishes between control dispensaries and treatment dispensaries. Each village reported the dispensary that villagers would attend for contraception. If that dispensary was also frequented by women who were assigned to the treatment (receiving CBD visits), that dispensary is characterized as a "treatment dispensary." (8 of the total 10 dispensaries).

It is important to note that all forms of contraception in Tanzanian public dispensaries are offered to women free of charge. In the baseline focus group discussions, most women reported that they did not know that contraceptives were free.

The empirical analysis in the study exploits the random assignment of individuals to the two treatment groups or to the control group to directly measure the treatment effect. Selection bias of the estimate of the impact of the program is reduced by the fact that individuals did not self-select into village treatment assignment.

[13] In Village 10, all three CBDs gave birth during the course of the intervention. One CBD gave birth to triplets and was not able to perform most of her work duties to visit households in her village. Another CBD was married to the Village Executive Officer, who was accused of corruption during the intervention. She was reluctant to visit households in her village during the public accusation. Village 10 is also the largest and most populated village in the study sample (400 households across five sub-villages).

2.4 Empirical Strategy

I first estimate the effect of the offer of the program on the study population. This estimation, known as the intent-to-treat (ITT) effect, measures the effect of being in a treatment village on contraceptive use and pregnancy. It does not distinguish between those who complied with the treatment assignment (living in a treatment village and participating in CBD consultations) and those who did not comply (living in a treatment village but not participating in consultations). Thus it is an average effect for these two groups.

The ITT estimation uses dichotomous outcome variables, so I use a linear probability model (LPM) to estimate the following multivariate regression: [14]

$$y_i = \beta_0 + \beta_T T_i + X_i'\beta + \epsilon_i \tag{2.4.1}$$

where y_i represents usage of contraceptives or pregnancy for individual i, T_i is an indicator variable for whether a household was offered the treatment,[15] X_i is a vector of household and individual control variables,[16] and ϵ_i captures all unobservable individual or household factors that may influence the outcome variable, y_i. If no individuals in the control group participated in the treatment then the estimate of β from this regression is a consistent and unbiased estimate of ITT (impact of offering the treatment). For the impact of T_i to be causal on y_i, all (unobservable) factors that are not in X (and thus are in the error term ϵ), must not be correlated with treatment, T_i. In other words, it must be that $E[T\epsilon] = 0$. Because the assignment to treatment in this study was done through a random number generator that is not based on village or household

[14] Although LPM may produce predicted values outside of [0,1], I use this estimation strategy because it does not impose a functional form on the error term. Moreover, I do not make forecasts on the outcome variables.

[15] This ITT estimation is also performed with dosage (number of CBDs/village population) as the treatment variable.

[16] The control variables include the following baseline data: wife's age, wife's age squared, female off-farm labor, male off-farm labor, wife is over the age of 40, contraceptive use in 2012, husband has been abusive, number of children born, number of children born squared, wife has completed primary school, standardized agricultural income, village population size, husband's desired fertility, wife dislikes family planning, husband wants at least 2 more children than wife, village-level stratification, distance to dispensary, wife wants no more children.

characteristics, the estimate of β_T is an unbiased estimate of the impact of T_i.

Although villages were randomly assigned to treatment, it is possible that women who complied with participating in the treatment (consulted with the CBD) were different in some unobservable way from those assigned to treatment who did not comply.[17] The varying levels of treatment compliance across villages (from 23 percent in Village 10 to 94 percent in Village 2) make the local average treatment effect (LATE) an appropriate parameter for treatment impact estimation.[18] The LATE parameter estimate measures the treatment effect specifically for those who chose to comply with the treatment, that is, those for whom the offer of the treatment persuaded them to obtain the treatment (and who would not choose the treatment if it were not offered). In this case, this means that the estimated treatment effect pertains to a sample of couples that are more likely to invite the CBD into their home.

To estimate the LATE, I measure the effect of P (actual participation in the treatment), instrumented with assignment to treatment (T), using the following first stage equation:

$$P_i = \beta_0 + \beta_T T_i + X_i'\beta + u_i \tag{2.4.2}$$

The instruments, in the vector T_i, include village treatment assignment and village level dosage of CBD treatment (3 CBDs/village population) to represent the varying level of household visits as a function of village population. I then use the predicted values of the treatment, P_i, to estimate the effect on contraceptive use in the following second stage equation:

$$y_i = \beta_0 + \beta_P \hat{P}_i + X_i'\beta + \epsilon_i \tag{2.4.3}$$

[17] For example, these could may contain husbands who are more willing to let a visitor speak privately to his wife about women's health.

[18] In essence, average treatment effect on the treated (ATT) and LATE require the same regressions. ATT includes a stronger set of assumptions and requires that the control group was not treated. In this case, 4.5 percent of the control group was treated, so the measurement is LATE. This spillover of treatment from the villages assigned to treatment to villages assigned to control may be due to CBDs wanting to share family planning information with couples in control villages or discrepancies over borders between treatment and control villages.

For this analysis to provide a causal and unbiased estimate of the effect of the treatment on the compliers (i.e. LATE), several assumptions must hold. First, the instruments, T_i must have relevant explanatory power for P_i. In other words, $Cov[T_i, P_i] \neq 0$. This can be tested by examining the combined significance of the instruments in the first stage equation. Second, the instrument must be exogenous to the second stage equation. In other words, $E[T_i' u_i] = 0$. Using the randomly implemented treatment variable (village treatment assignment) as an instrument for having actually been visited by a CBD is the key to the LATE estimation strategy. Treatment dosage (3 CBDs/ village population) is exogenous to the key intervention outcome, y_i, contraceptive use, as village population size was set prior to the intervention and is not related to village-level random assignment.

Next, I attempt to increase the precision of the estimate of the treatment effect by using Difference-in-Differences (DID) estimation. This econometric method accounts for any time-invariant unobservable baseline differences. I measure the DID treatment effect by estimating the following regression:

$$y_{it} = \beta_0 + \beta_1 T_i t + \beta_2 T_i + \beta_3 t + X_{it}^{'} \beta + \epsilon_i \tag{2.4.4}$$

where i represents individuals, T_i is an indicator variable for the treatment group, t is an indicator variable for the follow-up time period (2014) and ϵ represents any other time-variant unobservable characteristics that may affect the outcome y_{it} (current pregnancy or current use of any type of contraceptives). In this case, β_1 captures the treatment effect because it is the coefficient of the interaction of both time and treatment. I also combine the DID method with LATE, instrumenting the interaction variable with treatment assignment, dosage and time. This gives an estimate that accounts for time trends, uses both baseline and endline data, and measures the effect of the treatment for those who were induced to participate in the consultations by treatment assignment.

Finally, to explore the effect of violence with the treatment effect on fertility outcomes, I estimate a triple difference regression. I expand on 2.4.4, the double difference, by also including a dummy variable, V_i, for whether household i was abusive at baseline (2012). Using this strategy, I estimate the following regression:

$$y_{it} = \beta_0 + \beta_1 T_i t + \beta_2 T_i + \beta_3 t + \beta_4 V_i + \beta_5 V_i T_i + \beta_6 V_i t + \beta_7 T_i t V_i + X_{it}^{'}\beta + \epsilon_i \quad (2.4.5)$$

In this triple differences estimation, $\beta_1 + \beta_7$ represents the total treatment effect for women with abusive husbands, while β_1 by itself is the effect for women whose husbands are not abusive. Throughout the results, standard error estimates in this analysis are clustered at the village level. And due to the small number of clusters (12 villages), I employ the wild cluster bootstrap technique to adjust for potential over-rejection (Cameron and Miller, 2015).

2.5 Descriptive Statistics

The Meatu District, in rural Shinyanga region, is poor even by Tanzanian standards. Almost every home has dirt floors (98 percent) and only 1 percent have public electricity in the dwelling. Descriptive socioeconomic statistics across the control group, individual treatment group and couples treatment group are shown in Table 2.1. It is rare for women to work for pay outside of the family farm. In this analysis, I define "off-farm work" for both men and women as having employment or income outside of working within the family home or farm. Selling goods at a market or in the village, working as hired labor and teaching primary school are all examples of off-farm work in Meatu villages. Perhaps surprisingly, most households do not identify as religious, although they may still maintain traditional animist beliefs. As another sign of poverty, the majority of households in the study (77 percent) use unprotected improvised wells as a source of drinking water. This is the least sanitary option in this region because livestock and wild animals can drink from and defecate in these water sources.

T-tests were performed across the three groups to measure statistical difference across the three groups. For most variables, the difference in means is not significant; however, the difference for husbands' desired number of additional children is significant. The average number of children born per woman across all groups (around 5) is high

and the local rate of child mortality (around 14 percent of children born) is also very high. A high infant mortality rate is evidence that parents may view child rearing as a risky investment and want greater numbers of children to compensate for the high risk of child death. This table also shows the difference between women's desired number of additional children, her perception of her husband's desire for additional children, and his actual desired number of children. The average difference between the number of additional children desired by a wife and her husband is 2.5 children. The wife's perception of her husband's desired number of additional children is, on average, larger than her desired number of additional children and much smaller than his desired number of additional children. While it is clear that women want fewer children than their husbands, and that they are not able to estimate their partners desires, they appear to know to some degree that their husbands prefer larger families.

Intimate partner violence is unfortunately common among this population (36 percent on average) and is likely underreported. Women's off-farm employment and husbands' alcohol consumption are both positively correlated with physical abuse. The 2012 levels of violence indicate her expectations on the probability that he will inflict violence in attempt to induce her to not take contraceptives. This is represented by the probability of punishment, π in the model; I will measure how the effect of the family planning program changes based on previous level of violence.

2.6 Results and Discussion

2.6.1 Attrition

A number of households could not be traced for the follow-up household interview, either because they refused to participate or due to migration. The rate of attrition in this experiment is 16 percent in the women's survey and 21 percent in the men's survey. In cases of spousal separation, interviewing the woman was prioritized for the second round of the household survey. The rate of attrition varies across villages. Villages 11, 5 and 6 had the highest men's attrition rates at 23 percent. Village 1 and 8 had the

lowest attrition men's rates at 8 percent. The final sample size is 559 households.

Attrition did not occur randomly on observable characteristics. The 2012 rate of contraceptive use among those who did not attrit is 13 percent, while the rate of contraceptive use for the attritted households is 9 percent, although this difference is not statistically significant. Attrition levels vary slightly by treatment status: 16 percent attrition in the control group, 16 percent in the individual treatment group and 13 percent in the couples treatment group (differences not statistically significant). However, the attritted households were on average further from dispensaries (by 0.06 km, $t = 1.58$), contained women who were less educated (7.4 percent less primary completion, $t = 2.01$) and were slightly less likely to have women working off the farm (by 6.6 percent, $t = 0.26$). This slightly different attrition patterns by treatment groups make it impossible to completely rule out observable and unobservable differences between treatment and control households; yet, the estimate of the impact of the treatment on fertility behavior is unlikely to suffer from substantial bias due to differential attrition.

2.6.2 Longitudinal Changes in Family Planning

Changes in contraceptive use pre- and post-intervention can be seen in Table 2.2. Across all groups, I show the levels of contraceptive use in 2012, the change in current use of contraceptives, changes in pregnancy rates and changes in men's fertility preferences.

The percent of woman who are pregnant dropped over the course of the treatment and this drop is larger in the treatment groups. However the increase in the use of contraception is spread across all groups. The percent of women who were using contraception in 2012, before the intervention in the control, individual and couples treatment groups were 14 percent, 15 percent and 10 percent, respectively. This percentage increased in 2014 to 29 percent, 19 percent and 22 percent, respectively. While this shows a clear increase in the usage of contraceptives, this increase is surprisingly present in the control group as well as in the treatment groups. I discuss possible explanations for this increase in reported contraceptive use in the next section. The household survey data from 2012 provide insights into the main drivers of contraceptive use. Formal education,

work status (having an off-farm income) and a larger number of living children increase the likelihood that a woman had ever used contraceptives in 2012.

A second type of data was collected over the course of the intervention to gain insight into the fluctuations in village-level contraceptive use during process of bargaining over fertility (Figure 2.3). The community-based distributors (CBDs) collected monthly data as they visited each household, thus the observations include only the two treatment groups. The intervention data were recorded for 40 of the households that the CBD visited each month. In many cases, the data were from a different set of 40 households each month (e.g., January was sub-village 1, February was sub-village 2). As a result, the fluctuations observed in Figure 1 are mostly a result of the heterogeneous sampling of observations each month. However, contraception adoption and subsequent abandonment are also common over the course of women's fertility life course. Both the couples and individual treatment groups appear to be increasing their use of contraceptives, although at differing rates.

2.6.3 Estimation of Treatment Impact

In this section, I first explore the effect of the program's possible reduction in the psychosocial cost of contraceptives and the impact it has on fertility behavior. This exploration involves measuring the average effect of both treatment groups. I then measure the effect of the individual and couples treatment groups separately, to better understand the effect of the inclusion of husbands in consultations about family planning. And finally, I measure heterogeneity of the treatment effects by differences in expectations about the husband's behavior change the treatment effects.

Psychosocial Cost of Contraceptives

I begin by exploring the treatment effect on fertility behavior, which would suggest a reduction in the psycho-social cost of contraceptives through the family planning program. Table 2.3 shows the negative effects of any treatment (including both couples

and individual treatments) on pregnancies. For the entire study population, Column (1) shows that pregnancies decreased by an average of 6.4 percentage points, and this effect is statistically significant at the 1 percent level. In column (2) of Table 2.3, I measure the intent to treat effect by estimating the effect of an individual being assigned to the individual or couples treatment group on contraceptive use.[19] The ITT is negative but not statistically significant. Columns (3) and (4) of Table 2.4 show the estimation results for the sub-population of individuals that chose to comply with treatment assignment, or the LATE. Column (3) shows that the instruments (village treatment assignment and dosage (a function of village size)) are significant predictors of whether a household was treated (any type of treatment) (F-statistic=76.3). In column (4), the predicted values of the treatment, $\hat{P}_i$ are used to estimate the local average treatment effect on pregnancies. However, despite the more precise measurement of the effect of the treatment on the subpopulation of compliers, the coefficient in column (4) of Table 2.3 is still negative and not statistically significant. Under the difference-in-difference estimation strategy, which is a more precise measure of ITT using OLS, the effect of the combined treatment (as an interaction between treatment village and time) is also negative and not statistically significant. Finally, the local average treatment effect, using difference-in-differences estimation, is negative and statistically significant at a 10 percent level. Using the LATE instruments and the interaction of time and treatment, women in the treatment group are 14.3 percentage points less likely to be pregnant in 2014. This table provides support for the program's impact on the reduction of excess pregnancies.

Table 2.4 gives results confirm that much of the descriptive observations in Table 2.2, there has been a population-wide increase in reported contraceptive use. In Column (1), the effect of time on uptake of contraception is large and significant. Because the control group increased their use of reported contraceptive use more than the treatment groups, the intent-to-treat effect in column (2) is negative (although not statistically significant). Column (4) and (5) demonstrates the negative effects using LATE and DID. In the DID specification, women in the treatment group are 8.3 percentage points less

[19] I also measure the ITT effect of dosage (number of CBDs/village population) on contraceptive use and pregnancies. These results are similar to the estimation of the impact of treatment assignment, negative and not statistically significant.

likely to report contraceptive use at endline and this effect is statistically significant at the 10 percent level. This table demonstrates negative effect of any treatment (couples and individual treatment) on reported contraceptive use.

How is it possible that reported contraceptive use increased in all groups yet pregnancies dropped only in the treatment group? One possible explanation is a bias in self-reported contraceptive use. The process of enumeration about family planning and the larger focus on improving maternal health in the district could influence respondents' reported answers about contraceptive use and pressure respondents to indicate that they are using contraceptives when they are not. In other words, responses may be subject to desirability bias. Pregnancy, on the other hand, is less likely to be biased and is more easily observed.[20] A second explanation is that the substantial reduction in pregnancies in the treatment groups, amid reported increases in contraceptive use in the entire study sample, provides evidence of a possible lagged dispersion of contraceptive behavior from the treatment group to the control group. It is possible that the women in the control group have just began use of contraceptives, are not using the contraceptive methods properly, or are using them inconsistently. If this theory were true, we would expect to see a reduction in pregnancies in the control group in a later time period. The reduction in pregnancies as a result of treatment is in line with the predictions of the conceptual framework. A decrease in the psychosocial cost of adopting contraceptives (c_c) was predicted to increase use of contraceptives and reduce pregnancies. Improvements in knowledge, reduction in social stigma, and an increased public dialogue around family planning all decreased the psychosocial cost of family planning.

Effect of Including Husbands on Fertility Decisions

The second research question relates to the effectiveness of including husbands in family planning consultations. Table 2.5 shows the results of separated individual and couples treatment effects on pregnancies and demonstrates that, across all specifications, the individual treatment effect is negative. In the LATE+DID specification, women assigned

[20] If anything, pregnancies are likely under-reported as many women are not confident of pregnancies in the first trimester and may experience miscarriages after reporting pregnancies.

to the individual treatment decreased their pregnancies by 16.5 percentage points; this effect is statistically significant at the 5 percent level. Although the couples treatment appears also to have a negative effect, it is never statistically significant. Table 2.6 shows the results of separated individual and couples treatment effects on contraceptive use. The effect of the couples treatment on reported use is mixed. Under single difference ITT and single difference LATE, the effect of the couples treatment on contraceptive use is negative. However, the effect of the couples treatment is positive under double difference ITT and double difference LATE. Yet, it is not statistically significant under any of these specifications. The effect of the individual treatment on reported contraceptive use is consistently negative across specifications. The estimated local average treatment effect (LATE) for women in the individual treatment is an 18.2 percentage point reduction in reported contraceptive use (statistically significant at the 5 percent level).

The validity of reported contraceptive use data is again called into question given the negative effect of the individual treatment on pregnancies. Pregnancies are likely less subject to reporting bias than contraceptive use. The individual group also received consultations about family planning without their husbands' involvement and potentially without their husbands' approval, so in the case of covert use of contraceptives, these women may be less willing to report contraceptive use to strangers, including enumerators.[21] The statistically significant negative effect of the individual treatment on pregnancies suggests that family planning consultations are more effective without husbands' involvement. However, if we examine the effect of the couples treatment on husbands' fertility desires, relative to the individual group and the control group, Table 2.8 demonstrates that the involvement of husbands had a negative but insignificant effect. It is possible that while excluding husbands may lead to fewer births, including husbands may reduce their highly influential fertility desires.

In addition to the quantitative household and intervention data, I also collected qualitative data through focus group discussions in both 2012 and 2014. The most intriguing of these discussions was with the family planning community-based distributors (CBDs) after the intervention was complete. These women had essentially facilitated

[21] This is possible even though the enumerator interviews for the household survey take place in private.

family planning learning and experienced bargaining over fertility within their own villages. Both CBDs who implemented the individual treatment and those that implemented the couples treatment insisted that including husbands in the consultations is much more effective for education. According to one distributor: "If both husband and wife are involved in the CBD meeting, then the start of the conversation is even and men don't have all the power. They will continue to discuss family planning together and it is easy for them to reference what they learned from the CBD." This observation supports reproductive health policies that build on the couples intervention and intentionally include husbands in conversations about family planning.

The second research question also brings up two testable hypotheses. Whether to include husbands in family planning conversations depends on expectations of violence in the household. The individual treatment allows women covert information about contraceptives. While they appear to reduce births as a result of the treatment, how does this effect change as a result of husbands' abusive behavior? These testable hypotheses from the conceptual framework are: $(2a)$ When women do not expect husbands to be abusive ($\pi = 0$), they would be more likely to adopt contraceptives and reduce pregnancies and $(2b)$ when women do expect abuse from husbands ($\pi = 1$), women would be less likely to adopt contraceptives and reduce pregnancies.

This test involves estimating the effect of the individual treatment group on key outcomes. When a husband is not involved in family planning consultations, his ability to explicitly prohibit contraceptive use is limited. Here, I interact baseline spousal violence with treatment status and time to observe how violence expectations change the effect of the treatment. The results are shown in Table 2.7. The interaction effect of the individual treatment, post and violence is negative and statistically significant at the 5 percent level. This indicates that the total treatment effect on pregnancies that was observed in column (4) of Table 2.5 is primarily driven by women with abusive husbands taking advantage of the family planning information and reducing pregnancies. When women expect husbands to be violent, ($\pi = 1$), they are actually more likely to privately adopt contraceptives and reduce births. This effect is statistically significant at the 5 percent level. These results contradict the predictions from the conceptual framework.

Table 2.8 shows the effect of the individual and couples treatments on husbands' fertility desires again using a triple difference. In column (1), the couples treatment reduces husbands' fertility preferences, though not in a statistically significant way. In column (2), the individual treatment is also shown to increase husbands' fertility preferences, though again, not in a statistically significant way. This indicates that the joint educational conversations with the CBD may have changed the husbands' demand for children. Column (3) shows that the individual treatment had a statistically significant effect in reducing the number of spousal discussions about family size. The effect of the couples treatment is positive on discussions about family size, but not statistically significant. The individual treatment may reduce unwanted pregnancies significantly and this is likely to improve private welfare for the wives (who have smaller fertility desires). This strategic behavior is consistent with the non-cooperative model. Yet, the reduction in the husbands' fertility preferences may change the household model away from strategic behavior and individual best responses towards one of cooperation. This is evidence of an ongoing trade-off between individual welfare gains and cooperative value in marriage.

There are clear policy implications of the above dual treatment effects. The two ways of providing family planning information need not be exclusive. In cases of starkly heterogeneous fertility preferences and low intra-couple bargaining power for women, educational consultations should be joint and provided at the household level. However, a simultaneous informational distribution program about cost and access held privately for women would meet their immediate demand for contraceptives. A dual program, providing both joint education and individual information, would potentially both align preferences for cooperation in the long run and reduce excess fertility in the short run.

2.7 Conclusion

The experiment described in this paper provides evidence of the positive effect of a community education program in reducing unwanted births in an area of high fertility. The process of training community-based distributors (CBDs) and employing them to

visit households and discuss contraceptive options, reduced the psycho-social cost of fertility control for women and resulted in fewer pregnancies. The effect of the program is nuanced, though, in the relationship between reported contraceptive use and reduced pregnancies. Over the two-year study time period, reported contraceptive use increased substantially across both treatment groups and the control group. However, the family planning program reduced pregnancies in the in the treatment group by 14.3 compared to the control group, a difference that is statistically significant.

The decrease in treatment group pregnancies combined with a study-wide reported increase in contraceptive use allows for several potential explanations. First, reported contraceptive use is subject to bias. In the individual treatment group, women reduced pregnancies by 16.5 percentage points despite an insignificant effect on contraceptive use. However, pregnancies are less subject to reporting bias, thus the reduction in pregnancies in the individual treatment is more credible than the insignificant effect on contraceptive use. This group of women received the family planning consultations without their husbands' participation, which allows for concealed adoption of contraceptives. Given the potential for concealed use under asymmetric information, these women may be less willing to report their contraceptive use, both to their husbands and to enumerators. Additionally, given the lack of reduction in pregnancies in the control group and the reported increase in contraceptive use, it is possible that contraceptive use is being over-reported for this group. During the process of survey interviews at baseline and endline, respondents may have felt pressure to report the use of contraceptives, even when they were not. Enumeration does not exist within a vacuum and it is entirely possible that the presence of a research project may influence survey responses in a way entirely distinct from the intervention. A second potential explanation is a lagged dispersion of contraceptive behavior from the treatment group to the control group. Households who were visited by a CBD over the course of the treatment may have been able to share this information with neighboring villages, and the effect of the treatment spilled over to non-treated households. At the time of endline enumeration, women in the control group may have just begun using contraceptives, or may not yet be using them properly or consistently.

The solution to whether husbands should be involved in consultations about family

planning depends on policy objectives and preferences. Women who consulted with the family planning workers individually (without their husbands) had a larger reduction in pregnancies than those who consulted together with their husbands. However, the involvement of husbands in these discussions had the effect of reducing husbands' fertility preferences. These fertility desires are very influential on high fertility rates (Ezeh, 1993). The reduction in fertility desires was small and not significant for the entire population, but much larger and significant when interacted with the presence of abuse. In a region of the world where women have limited bargaining power within the household, private information about contraceptives can afford women fertility control and improved welfare. Meanwhile, joint consultations with an informed family planning worker may improve cooperation through aligning preferences for children. The non-cooperative model predicts that, given the opportunity, women will take advantage of private information and delay births. The model prediction that, when women expect abuse from their husbands, they will be less likely to adopt contraception is rejected by the the empirical evidence. In fact women with abusive husbands who receive private information about family planning are more likely to adopt contraceptives and reduce fertility.

This paper contributes to the literature by bridging the microeconomics literature on asymmetric information and strategic behavior among spouses (Ashraf, Field, and Lee, 2014; Castilla and Walker, 2013) and the demography literature on spousal communication and education as vehicle for family planning (Ezeh, Seroussi, and Raggers, 1996; Lasee and Becker, 1997). The long intervention allows for intra-household bargaining over fertility. In the unresolved question on whether husbands should be included in family planning education, my results provide evidence of the positive cooperation effects of inclusion while also supporting private welfare gains for women in individual consultations. Further research on the balance of husbands' and wives' preferences during the bargaining process would lead to a better comparison of these trade-offs.

This randomized field experiment is small in scale: the intervention included 24 family planning workers across eight villages in one district in Tanzania. After attrition, the sample included two treatment groups and a control group of about 150 households each. Yet, the study provides substantial support for the effectiveness of

community-based distribution of family planning services in reducing excess fertility. The trade-off between improved fertility control for women on one hand, and improved communication and aligned preferences on the other, supports policy interventions that include both joint and individual informational sessions. The inclusion of husbands in education-focused consultations may reduce husbands' fertility preferences, and the exclusion of husbands in information and access-based consultations may allow women to meet immediate demand for fertility control. In areas of the developing world with high fertility rates and starkly different spousal fertility preferences, community-based distribution of family planning information plays an important role in reducing unwanted pregnancies.

2.8 Figures

Figure 2.2: Treatment and Control Households in Meatu District

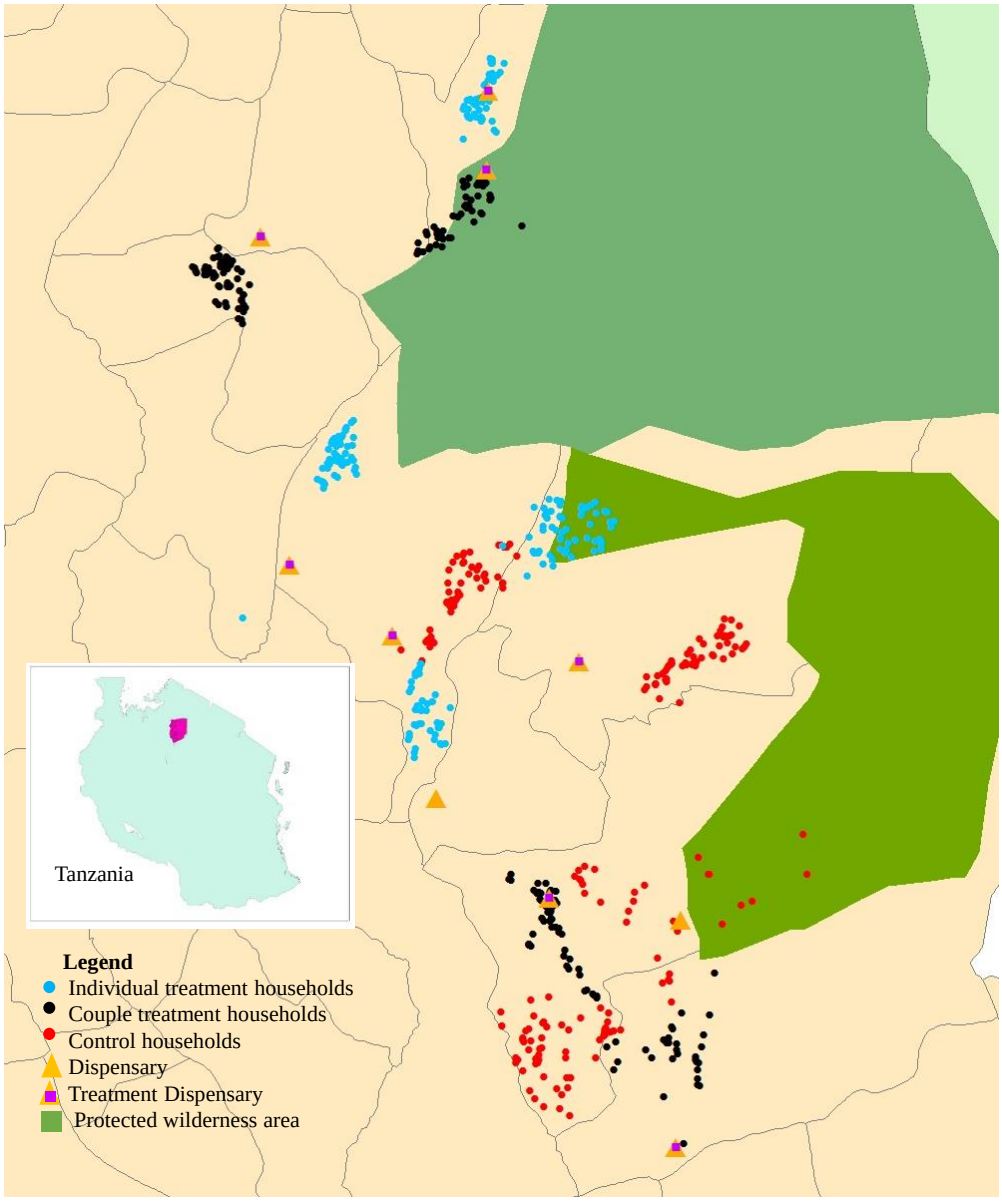
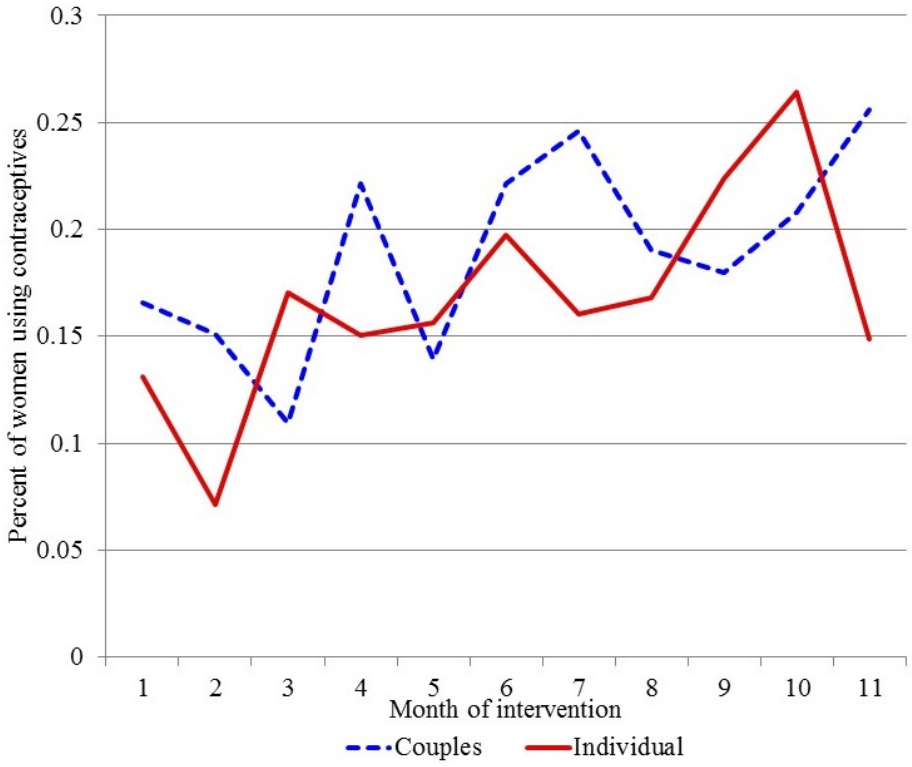


Figure 2.3: Dynamic Contraceptive Use by Treatment Type



2.9 Tables

Table 2.1: Sample Characteristics and Balance

VARIABLES	(1) Control	(2) Indiv. Treat.	(3) Couples Treat.	(4) P-value
Wife has off-farm income	0.08	0.04	0.08	0.78
	(0.02)	(0.02)	(0.02)	
Not religious	0.70	0.60	0.72	0.79
	(0.04)	(0.04)	(0.04)	
Distance to dispensary (km)	0.77	0.66	0.37	0.08*
	(0.02)	(0.03)	(0.02)	
Unprotected well for water	0.96	0.73	0.63	0.14
	(0.02)	(0.04)	(0.04)	
Number of bikes in hh	1.30	1.28	1.27	0.54
	(0.06)	(0.06)	(0.07)	
Rainy season ag income (USD)	833.85	549.39	857.91	0.91
	(74.53)	(38.99)	(55.07)	
Number cattle owned	18.49	9.18	24.78	0.98
	(2.37)	(0.99)	(7.60)	
Number children born per woman	5.18	5.46	5.13	0.22
	(0.24)	(0.23)	(0.27)	
Num. children died per woman	0.74	0.77	0.65	0.56
	(0.12)	(0.08)	(0.09)	
Wife desired num. add'l children	1.68	1.43	1.30	0.16
	(0.20)	(0.16)	(0.15)	
Wife's view of husb. desired num add'l chrn	2.38	1.72	1.17	0.08*
	(0.25)	(0.18)	(0.15)	
Husband's desired add'l children	4.42	3.27	3.59	0.42
	(0.41)	(0.29)	(0.29)	
Husband has ever been abusive towards wife	0.40	0.40	0.35	0.12
	(0.04)	(0.04)	(0.04)	
Wife has hidden contraception from husb.	0.07	0.03	0.04	0.32
	(0.02)	(0.01)	(0.02)	
Wife has completed primary school	0.90	0.83	0.92	0.44
	(0.02)	(0.03)	(0.02)	
Observations	146	157	144	

Cluster robust standard errors in parentheses using Cameron and Miller (2015) bootstrapping for a small number of clusters.

Table 2.2: Longitudinal Changes in Family Planning

	2012			2014		
VARIABLES	Ctrl.	Indiv. Treat.	Coup. Treat.	Ctrl.	Indiv. Treat.	Coup. Treat.
Woman is using any type contraception	0.14	0.15	0.10	0.29	0.19	0.22
	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)
Wife currently is pregnant	0.19	0.21	0.26	0.18	0.13	0.16
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Wife ever used family planning in 2012	0.19	0.18	0.15			
	(0.03)	(0.03)	(0.03)			
Husb desired add'l children, 2012	4.99	3.73	3.80	3.08	2.71	2.76
	(0.40)	(0.28)	(0.27)	(0.30)	(0.22)	(0.26)
Observations	177	195	183	163	186	165

Standard errors in parentheses

Table 2.3: Any Treatments: Effect on Pregnancies

VARIABLES	(1) Single difference Time Effect	(2) Single diff. ITT	(3) Single diff. 1st stage	(4) Single diff. LATE	(5) Double diff. ITT	(6) Double diff. LATE
Post	-0.064***				-0.030	-0.016
	(0.023)				(0.066)	(0.047)
Assigned to treatment		-0.019	0.439**		0.040	
		(0.029)	(0.189)		(0.059)	
Dosage of CBDs in vill.			13.724**			
			(5.892)			
Participated in treatment				-0.015		0.120**
				(0.043)		(0.054)
Treatment village*Post					-0.074	
					(0.078)	
Participated*Post						-0.143*
						(0.076)
Controls?	No	Yes	Yes	Yes	Yes	Yes
Observations	1,066	560	560	560	1,066	1,066
R-squared	0.007	0.062	0.332	0.052	0.068	0.065

Control variables include: wife's age, wife's age squared, female off-farm labor, male off-farm labor, wife is over the age of 40, contraceptive use in 2012, husband has been abusive, number of children born, number of children, born squared, wife has completed primary school, standardized agricultural income, village population size, husband's desired fertility, wife dislikes family planning, husband wants at least 2 more, children than wife, number of wives, village-level stratification, distance to dispensary, wife wants no more children.

Cluster-robust standard errors in parentheses using Cameron and Miller (2015) bootstrapping for small number of clusters.
*** p<0.01 ** p<0.05 *p<0.1

Table 2.4: Any Treatments: Effect on Contraceptive Use

VARIABLES	(1) Single diff. Time Effect	(2) Single diff. ITT	(3) Single diff. 1st stage	(4) Single diff. LATE	(5) Double diff. ITT
Post	0.095***				0.141***
	(0.023)				(0.000)
Assigned to treatment		-0.079	0.439**		0.010
		(0.076)	(0.189)		(0.009)
Dosage of CBDs in vill.			13.724**		
			(5.892)		
Participated in treatment				-0.119	
				(0.082)	
Treatment village*Post					-0.083*
					(0.047)
Controls?	No	Yes	Yes	Yes	Yes
Observations	1,066	560	560	560	1,066
R-squared	0.015	0.180	0.332	0.160	0.362

Control variables include: wife's age, wife's age squared, female off-farm labor, male off-farm labor, wife is over the age of 40, contraceptive use in 2012, husband has been abusive, number of children born, number of children, born squared, wife has completed primary school, standardized agricultural income, village population size, husband's desired fertility, wife dislikes family planning, husband wants at least 2 more, children than wife, number of wives, village-level stratification, distance to dispensary, wife wants no more children.

Cluster-robust standard errors in parentheses using Cameron and Miller (2015) bootstrapping for small number of clusters.
*** p<0.01 ** p<0.05 *p<0.1

Table 2.5: Separate Treatments: Effects on Pregnancies

VARIABLES	(1) Single difference ITT	(2) Single diff. LATE	(3) Double diff. ITT	(4) Double diff. LATE
Post			-0.067	-0.019
			(0.070)	(0.049)
Participated in couples treatment		-0.025		0.047
		(0.043)		(0.040)
Participated in indiv. treatment		0.001		0.121**
		(0.055)		(0.054)
Participated in couples * Post			-0.029	-0.074
			(0.056)	(0.063)
Participated in indiv. * Post			-0.016	-0.165**
			(0.178)	(0.074)
Assigned to couples treat	-0.019		0.024	
	(0.025)		(0.038)	
Assigned to indiv. treat	-0.018		0.003	
	(0.049)		(0.089)	
Controls?	Yes	Yes	Yes	Yes
Observations	560	560	1,066	1,066
R-squared	0.062	0.054	0.064	0.064

Control variables include: wife's age, wife's age squared, female off-farm labor, male off-farm labor, wife is over the age of 40, contraceptive use in 2012, husband has been abusive, number of children born, number of children, born squared, wife has completed primary school, standardized agricultural income, village population size, husband's desired fertility, wife dislikes family planning, husband wants at least 2 more, children than wife, number of wives, village-level stratification, distance to dispensary, wife wants no more children.
Cluster-robust standard errors in parentheses using Cameron and Miller (2015) bootstrapping for small number of clusters.
*** p<0.01 ** p<0.05 *p<0.1

Table 2.6: Separate Treatments: Effects on Contraceptive Use

VARIABLES	(1) Single diff ITT	(2) Single diff LATE	(3) Double diff ITT	(4) Double diff LATE
Post			0.092***	0.135***
			(0.000)	(0.034)
Participated in couples treatment		-0.040		0.030
		(0.067)		(0.026)
Participated in indiv. treatment		-0.182**		0.047
		(0.093)		(0.054)
Participated in couples * Post			0.006	-0.051
			(0.018)	(0.048)
Participated in indiv. * Post			-0.041	-0.155
			(0.049)	(0.124)
Assigned to couples treat	-0.040		-0.018	
	(0.063)		(0.029)	
Assigned to indiv. treat	-0.105		-0.031*	
	(0.075)		(0.017)	
Controls?	Yes	Yes	Yes	Yes
Observations	560	560	1,066	1,066
R-squared	0.184	0.165	0.360	0.358

Control variables include: wife's age, wife's age squared, female off-farm labor, male off-farm labor, wife is over the age of 40, contraceptive use in 2012, husband has been abusive, number of children born, number of children, born squared, wife has completed primary school, standardized agricultural income, village population size, husband's desired fertility, wife dislikes family planning, husband wants at least 2 more, children than wife, number of wives, village-level stratification, distance to dispensary, wife wants no more children.

Cluster-robust standard errors in parentheses using Cameron and Miller (2015) bootstrapping for small number of clusters.
*** p<0.01 ** p<0.05 *p<0.1

Table 2.7: Changing Treatment Effects Under Violent Behavior Expectations

		Triple Difference Pregnancies
VARIABLES	Couples	Individual
Violence * Post * Indiv		-0.173**
		(0.074)
Violence * Post * Couples	0.100	
	(0.106)	
Post	0.020	-0.039
	(0.032)	(0.058)
Participated in couples treatment	0.018	
	(0.037)	
Viol husb * Coup treat	-0.007	
	(1.304e+19)	
Participated in couples * Post	-0.073	
	(0.084)	
Violence * Year	-0.157**	-0.018
	(0.068)	(0.076)
Viol husb * Indiv treat		0.177
		(0.127)
Participated in indiv. * Post		0.055
		(0.052)
Observations	688	720
R-squared	0.062	0.063

Control variables include: wife's age, wife's age squared, female off-farm labor, male off-farm labor, wife is over the age of 40, contraceptive use in 2012, husband has been abusive, number of children born, number of children, born squared, wife has completed primary school, standardized agricultural income, village population size, husband's desired fertility, wife dislikes family planning, husband wants at least 2 more, children than wife, number of wives, village-level stratification, distance to dispensary, wife wants no more children. Cluster-robust standard errors in parentheses using Cameron and Miller (2015) bootstrapping for small number of clusters. *** p<0.01 ** p<0.05 *p<0.1

Table 2.8: Effect on Husbands' Fertility Preferences

VARIABLES	Husband's Fert. Prefs. Couples	Individual	Discussion Whole Sample
Post	0.144	-1.131**	0.023
	(0.256)	(0.375)	(0.046)
Participated in couples treatment	0.005		0.028
	(0.212)		(0.036)
Participated in indiv. treatment		-0.234	0.125***
		(0.241)	(0.048)
Violence * Post * Couples	-0.290		-0.052
	(0.496)		(0.572)
Viol husb * Coup treat	-0.083		-0.911
	(0.149)		(1.300)
Participated in couples * Post	-0.208		0.047
	(0.421)		(0.082)
Violence * Post * Indiv		-0.147	-0.136
		(0.543)	(0.441)
Viol husb * Indiv treat		0.171	0.111
		(0.307)	(0.104)
Participated in indiv. * Post		0.939	-0.218***
		(0.576)	(0.081)
Observations	637	670	1,069
R-squared	0.748	0.690	0.251

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Chapter 3

Fueling Financial Literacy: Estimating the Impact of Youth Entrepreneurship Training

3.1 Introduction

The prevalence of high youth unemployment is a global problem. According to an International labor Organization (ILO) study in 2013, 73.4 million youth (aged 15-24) are affected by unemployment. Youth may experience their initial entry into the labor market usually during adolescence, which can set the stage for adult employment. Development organizations and governments are increasingly turning to entrepreneurship training as a strategy for reducing youth unemployment and alleviating poverty. While there are many factors that contribute to the success of youth when they finish training and enter the labor market, it is important to first understand whether the program itself had an impact on youth's ability to sustain employment or run a business. A comprehensive World Bank review of hundreds of youth job training program evaluations in developing countries concluded that there is weak evidence on labor outcomes for these youth (Betcherman et al., 2007). When positive effects of training on labor outcomes were present, the evidence was faint or inconsistent. This essay addresses that gap in

evidence by measuring the impact of a youth entrepreneurship program on the financial literacy and employment knowledge of marginalized Tanzanian youth.[1]

Small-scale businesses are the main source of income for hundreds of millions of people in the developing world (Dupas and Robinson, 2009). While most policy discussions about employment, entrepreneurship and microfinance often focus on credit constraints, the assumption around this discussion is that, subject to those constraints, entrepreneurs are managing enterprises optimally (Karlan and Valdivia, 2011). However, most small enterprise owners or self-employed in sub-Saharan Africa have no formal business training, entrepreneurship skills, or, in the case of marginalized youth, even a secondary education (Attanasio, Kugler, and Meghir, 2011; Mano et al., 2012). This has led to a growing interest among governments and development organizations in equipping impoverished youth with the entrepreneurship knowledge, financial literacy skills, and confidence in labor market navigation that would help secure employment or start their own enterprise. Generally captured in the notion of 'entrepreneurship training', particularly for those who have not completed formal education, such programs aim to strengthen basic literacy and numeracy, provide vocational and life skills and, in some cases, give participants experience saving money and accessing credit, often through group schemes.

The rise of these entrepreneurship training programs as the centerpiece of some organizations' poverty alleviation efforts represents, to a considerable extent, a shift in the underlying philosophy of international economic and social development efforts (Baxter et al., 2014). Previous poverty reduction strategies have been viewed as the responsibility of national governments, often assisted by bilateral and multilateral aid organizations. In sub-Saharan Africa, however, these government efforts at poverty reduction have not been particularly successful (Handley et al., 2009; Collier, 2007). The premise of many of the entrepreneurship training programs, on the other hand, is that the labor market may be successful in rewarding human capital improvements relative to other poverty-reduction government programs that may provide assistance without building skills (Klinger and Schündeln, 2011). In this approach, these training programs conduct market research to teach skills demanded by local industries, but individuals

[1] This chapter is coauthored with Brooke L. Krause and David Chapman.

end up bearing the responsibility for improving their own welfare. Once provided with the knowledge and skills relevant to entering and competing in the labor market, the presumption is that those receiving this training will secure employment in their community or start their own enterprise. While training efforts can increase the skills and knowledge of participants, it is not yet clear if these efforts will truly enable participants to successfully transition into the labor market.

To gain reliable insight into training program effects, the line of research assessing and solidifying empirical evidence must be extended. The question of great interest to many program sponsors and donors is the extent to which observed changes in an individual's labor market performance are due to participation in the entrepreneurship training program. Employment status and income are frequently used as the outcomes of interest in evaluation studies. In this context, employment is oftentimes casual, and inconsistent, while income can be difficult to measure (Sahn and Stifel, 2000). Employment status and income data were not collected from our sample, so we focus our analysis on measuring the intermediary mechanisms, such as financial literacy and employment knowledge. These confidence and ability indicators are important steps in realizing entrepreneurial and employment success.

A common shortcoming in evaluation is the challenge of establishing a program impact using observational data that lack a comparison group and instead assessing change over time among program participants. A number of time-variant factors may affect youth livelihood outcomes for reasons unrelated to the training, such as normal maturation, the influence of friends and family, and local, regional or national events that occur outside the program. It is widely understood that the gold standard in assessing whether training programs yield intended changes in participants' behaviors, knowledge, skills, and attitudes is a randomized control trial (RCT). There are both practical and financial reasons an RCT is difficult to implement in the context of a community-led training program. The development program evaluated in this paper is a locally-operated program called U-Learn, under the direction of the Non-Governmental Organization (NGO) Swisscontact. As in this case, the imposition of an RCT can undermine the implementation of the local operations of the program. For example, one goal of community-led training programs is local capacity-building and community ownership over the program

itself. The community is more likely to address youth unemployment without outside intervention in the future if they are involved in the design and implementation of this training program, including selection of youth participants.

From the perspective of an RCT, the spread of additional youth training programs into neighboring geographic areas may be considered contamination of the control group. However, the spread of other similar community-led programs that address youth unemployment is seen as a positive consequence of the U-Learn program. Through local ownership and participation, the program aims to improve community accountability for youth employment. For these reasons, the NGO and evaluation partners saw an RCT as not ideal, nor feasible in this context. With concerns over local ownership and management of the entrepreneurship program, the researchers, local partners, and funder opted for a mixed method evaluation design including primary data collection consisting of qualitative interviews, demographic participant data and a quantitative survey without randomization. Further, even the creation of an appropriate population of similar but non-participating youth in a comparison group has its problems. Trainee selection in most programs is not random; community program managers may select youth participants quite intentionally to favor either those most likely to succeed in the program or the most marginalized. Consequently, selecting from a demographically similar group of students misses the unobservable, and subtler, differences between participants and non-participants.

We make two key contributions to the existing literature. First, this paper builds on the existing methodological literature seeking rigorous empirical evaluation techniques to establish program impact when randomization is not feasible using primary data collected in rural Tanzania. Second, this paper provides greater insight into training program effects by establishing the positive effect of youth training programs on learning outcomes, such as financial literacy and employment knowledge. Knowledge of the labor market, job-search skills, employment confidence, and financial literacy, together comprise mediating skills and attitudes in the process towards gainful employment and enterprise ownership for youth.

The remainder of this paper is organized as follows. Section two reviews the literature on training programs and evaluation methodologies in a non-randomized setting. Section three explores the U-Learn youth entrepreneurship program in Tanzania and primary data collection methods. Section four discusses the strategy this paper uses for measuring program impact and the different sensitivity analyses conducted. Section five presents the empirical results from the various methods employed. Finally, Section six concludes by discussing the findings and policy implications.

3.2 Evidence on Youth Training Programs

While the philosophy of entrepreneurship or skills training programs is attractive to many governments and development organizations, the extent to which such training programs actually yield the intended benefits has yet been elusive (Oosterbeek, Van Praag, and Ijsselstein, 2010). The World Bank-sponsored review of 289 studies from 84 countries of interventions aimed at integrating youth into the labor market finds weak evidence in favor of positive labor market effects (Betcherman et al., 2007). Claims of program impact were often based on faint or inconsistent evidence. Card et al. (2011) find that while a randomized evaluation of a job-training program in the Dominican Republic revealed no positive impact on employability, the non-randomized evaluation methods (that did not include propensity score matching) did measure a positive impact of the program on the same outcomes. A USAID review of 54 research and evaluation studies published between 2001 and 2012 on the topics of youth employment, business development, school to work transition and youth entrepreneurship concludes that these programs in developing countries have a positive impact on employment and earnings, but also that the evaluation design of many of the studies was weak (United States Agency for International Development, 2013). Card et al. (2011) also note that rigorously evaluating job training programs is important to demonstrate the limitations of such programs in addressing the labor market barriers faced by disadvantaged youth. However, because workforce development and entrepreneurship training programs include various facets of implementation and take place across different location, trainers and cohorts, the inclusion of rigorous evaluation methods can be challenging. Given the

substantial amounts of funding now being directed to supporting such programs and the challenge of their evaluation, funders need a stronger evidence base.

Two seminal papers have established propensity score matching as a valid method to evaluate training programs. First, Heckman, Ichimura, and Todd (1997) analyse the possibility of devising a matching procedure for the evaluation of a prototypical job-training program that produces impact estimates close to those of a randomized social experiment. The authors find support for the estimation techniques that match individuals based on their propensity for participation in the training program. They also note the importance of having a control or comparison group that participates in the same labor market as training recipients. Dehejia and Wahba (1999) use the National Supported Work data (U.S. based training program) to evaluate the performance of propensity-score matching methods, including pairwise matching and caliper matching. The authors also confirm that matching estimators succeed in closely replicating the results in earnings obtained through an experimental evaluation of the program. Dehejia and Wahba (1999) conclude that matching approaches are, in fact, more reliable than traditional econometric estimators.

More recent studies of job-training programs, using various methods of evaluation, have yielded results that are less optimistic and can be difficult to interpret. McKenzie and Woodruff (2013) review the evaluation literature on training and entrepreneurship programs and find modest impacts of training on the survivorship of existing firms. However, they did find stronger evidence that training programs help prospective owners launch new businesses. In an RCT evaluation of a comprehensive business-training program in Peru, Karlan and Valdivia (2011) find that the treatment group of trainees had no change in profits, business revenue or employment within their small businesses within the time period of the evaluation. This two-year program included both business skills and strategy development for current business owners. Yet, despite the lack of changes in major business outcomes over the study period, the authors did observe improvements in business *knowledge* among trainees. One way to interpret these results is that improvements in knowledge of business operations is not sufficient to impact profits or employment, however, another interpretation is that the two-year time-frame of evaluation was an insufficient period of time to measure the impact of knowledge

improvements on business outcomes. Business knowledge and financial literacy skills may be intermediary mechanisms on the trajectory to employment.

Training programs that target marginalized populations (women, school dropouts or people out of the labor force) have had slightly more success. Attanasio, Kugler, and Meghir (2011) find evidence through a randomized control trial in Colombia that subsidizing vocational training for disadvantaged youth had a significant positive impact on earnings and the probability of employment for female participants, although it curiously had little impact on male participants. Field, Jayachandran, and Pande (2010) explore the imposition of social institutions and marginalization on women of various castes in India. They find that Hindu women who face severe social restrictions benefited the most (in terms of business income) from a basic financial literacy-training program. De Mel, McKenzie, and Woodruff (2014) specifically targeted entrepreneurial training in Sri Lanka towards women running subsistence businesses and those out of the labor force. Their results show that within a year of the program, women who received a grant and training as a packaged intervention had significant improvements in business profitability and that training was generally more effective for new business owners.

The De Mel, McKenzie, and Woodruff (2014) type of packaged skills development program, although extensive in its objectives, may have more success in improving the skills and income of participants. Deshpande and Zimmerman (2010) present existing evidence on the dual development potential of youth savings accounts, which not only promote access to credit and savings, but can induce financial behavioral change as well. The authors point to growth in microfinance and financial services in developing countries, but conclude that more research is needed on the role of savings accumulation, mentoring and skills training on the lives of youth. The present study addresses this gap in the literature by analyzing the impact that a comprehensive entrepreneurship-training program has on financial literacy and business skills development for marginalized youth in Tanzania.

3.3 The U-Learn Program and Youth Characteristics

This study assesses the extent to which an entrepreneurship-training program designed and implemented by Swisscontact, a Swiss NGO, was able to significantly increase relevant knowledge, skills, and attitudes of marginalized youth in rural Tanzania. This study draws on primary survey data collected from 434 youth who participated and completed the nine-month U-Learn program.[2] This program targets school dropouts aged 15-26 years old by supporting youth learning, earning and saving. Components of the program include: technical and entrepreneurship skills-training, internships, job placement, business start-up support, linkages to financial service providers, the formation of savings groups, and life skills counseling. This multi-faceted program with formal education, practical connections and internship opportunities comprises a packaged intervention approach.

This study was undertaken to assess the impact of the U-Learn program, which was locally operated in northwestern Tanzania.[3] The training program is being implemented in both rural areas, such as Nshamba, and urban areas, such as Bukoba. It includes elements focused on knowledge, vocational skill development, and life skills and is intended to lead to either employment in the labor market, self-employment or further education. This comprehensive program targets out-of-school youth and utilizes an apprenticeship model of technical and vocational training using community and business mentors and experts. Participants develop vocational skills through participation in relatively small learning groups (approximately 20 participants), and savings groups that are linked with financial service institutions. Youth form these self-governed savings groups, called Mavuno saving and lending groups, where they can earn interest on savings as well as offer micro-loans to one another.

The U-Learn youth entrepreneurship training program is nine months in duration, after

[2]The program did suffer from some attrition. 492 students began the program, which represents a 11.7% attrition rate. Cohort 1 had a 9.7% attrition rate, while Cohort 2 had a 13.1% attrition rate. Because we only have endline data from those who completed the program (and those who dropped out were likely less motivated to learn), our estimates may overstate the true impact of the program.

[3]More information on the Swisscontact U-Learn program can be found at http://www.swisscontact.org/en/projects-and-countries/project-finder/p/Project/show/u-learn-learn-earn-and-save.html.

which a new cohort was recruited and provided with training. The first two cohorts of youth who received this training are included in this paper. Cohort 1 was implemented in eight districts and Cohort 2 in ten districts, with six districts that offered programming to both Cohort 1 and Cohort 2 participants. It is important to note that the determination of Cohort 2 program locations was not based on youths' reported success during Cohort 1 and that village leaders were not led to believe this to be the case. Cohort 2 entered the program at the same time that Cohort 1 was completing the program. Figure 1 shows the cohort data collection timeline.

The selection criteria for program participation was non-random and consistent across cohorts; youth must be between 15 and 26 years of age and have not completed secondary school. Further, youth are selected by their community leaders based on their level of marginalization, which is determined during a one-on-one interview, and includes information on family life, household structure, types and sources of income. After this interview, some youth are screened out of the pool of candidates as a result of the selection criteria and program requirements, while others simply choose not to enter the program.

The data sample includes all training program participants. The survey was administered orally in Swahili, as there was considerable variation in the literacy skills among the youth. Community-based trainers involved in the program and familiar with the youth administered the survey program participants. All youth completed the survey as they were entering the program and then again when they finished the nine month training period. The enumeration process took approximately two months for each cohort. The survey includes questions that capture demographic information, employment skills, financial abilities, life skills, and availability of peer and adult social supports. It also includes more subjective aspects of individual character including the participants' values and goals, confidence in their skills and abilities, and beliefs about men's and women's financial and entrepreneurial abilities. The questionnaire can be viewed in Table 3.3.[4]

[4]One important feature of the questionnaire is that it addresses self-reported attitudes, skills and beliefs. Relative to a skills test of savings knowledge or financial literacy, these self-reported outcomes may be subject to social desirability bias.

3.4 Research Framework

Evaluating social programs using observational data is challenging, at best. Observational studies usually violate the ignorable treatment assignment assumption and thus, selection bias is assumed to be present in program participation (Rosenbaum and Rubin, 1983). In this setting, selection bias occurs when youth self-select or are selected into a program based on unobservable characteristics that lead them to be more likely to gain from the program (Smith and Todd, 2001). Although the youth are all disadvantaged, some may have unobservable characteristics, such as ambition and motivation, which led them to participate in the program in the first cohort (as compared with those in a subsequent cohort). The methods of ex-post program evaluation center on imputing the missing counterfactual: the outcomes that would have occurred in the absence of the program.

One widely used method of observational program evaluation is a comparison of the outcomes of program participants to similar matched non-participants to impute the counterfactual (Dehejia and Wahba, 2002; Abou-Ali et al., 2010; Todd, 2007). For a more formal consideration of imputing the counterfactual through propensity score matching, let us denote $E(Y_1|Z, \tau = 1)$ as the average employment or financial knowledge of youth in the first cohort ($\tau = 1$) after they have completed the training and $E(Y_0|Z, \tau = 0)$ as the average employment and financial knowledge of youth in the Cohort 2 ($\tau = 0$) before they have begun the treatment both conditional on Z, a vector of individual characteristics. The subscript on Y, the employment or financial knowledge outcomes, denotes the element of time (either before or after the program). We can define the treatment effect on the treated (ATT) as a mean difference: $E(y_1|Z, \tau = 1) - E(\hat{y}_0|Z, \tau = 0)$. In estimating the program's impact, the dilemma of not observing post-program outcomes of Cohort 1 had they not completed the program (the counterfactual) is resolved by examining the average outcomes of the a proper comparison group of youth from Cohort 2, before they began the training.

Propensity score matching offers a way to test for this potentially causal relationship: conditional on observed characteristics of the youth (Z), the program has an impact

on employment and financial skill outcomes. Using the imputed comparison group, propensity score analysis matches youth who have participated in the program with youth who are just entering the program to compare their learning, attitude, and saving outcomes. Previous research has noted that matching methods do nothing to correct for unobserved differences between treated and untreated observations (Hirano, Imbens, and Ridder, 2003; Smith and Todd, 2001). Despite this, there are some advantages of matching over ordinary least squares (OLS) regression analysis (Berk, 2004; Glewwe and Todd, 2015; Ravallion, 2007). First, the treatment effect on the treated (ATT) can be calculated without specifying a functional form of the effect, through a conditional mean, as opposed to a linear or quadratic relationship. Secondly, the performance of the ATT estimate is improved by imposing the condition of common support, avoiding forced and potentially bad matches. Lastly, matching youth based on observable characteristics emulates a random experiment to some degree by aligning the distribution of the observable characteristics in both the matched comparison and treatment groups (Glewwe and Todd, 2015).

Since participant selections were not random and no contemporaneous control group was available, this study uses propensity score analysis to address selection bias. We employ cross-sectional matching using a set of 36 observed characteristics, Z, under the assumption that outcomes are independent of program participation conditional on these observed characteristics. The main findings reported in this study applied the matching procedure using Epanechnikov kernel weights to match youth between Cohort 1 and Cohort 2. Kernel matching estimates the average treatment effect by nonparametric kernel regression where the weights are obtained through a multiplicative kernel using the Epanechnikov function. While there are multiple methods to use in matching, the Epanechnikov kernel method is becoming standard in the matching literature (Binzel and Assaad, 2011). To confirm the robustness of our results, we also report results from multiple matching techniques including nearest neighbor matching with and without replacement, ten nearest neighbors matching, and Mahalanobis matching (see Table 3.4). The standard errors for the Epanechnikov kernel method were bootstrapped (250 iterations) and were clustered by district. Because it is still unclear if bootstrapping is appropriate for nearest neighbor matching methods (Abadie and Imbens, 2008),

we only applied this standard error estimation only to the kernel, radius matching and Mahalanobis distance matching methods.

To construct a comparison group for this program, we matched youth who had already participated in the program (Cohort 1) with youth who had not yet participated in the program (Cohort 2). As discussed in the previous section, the educational, age and marginilization selection criteria for program participation were the same across cohorts. Because of the consistent (albiet subjective) selection criteria across the two cohorts, the youth in the second cohort who have not yet participated make a valid comparison group for the youth in the first cohort.

Despite the theoretical consistency of the selection process, it is still possible that the selection process performed by community leaders may have been different *in practice* between the two cohorts. The possibility of such bias necessitates a brief discussion of directionality. If the committees that selects youth participants (whether intentionally or not) choose youth that they believe would be most successful into the first cohort, this would imply that our results overestimate the true impact of the program. It could also be the case that the committee may have chosen youth to participate in the first cohort who they think would most benefit from it (perhaps they are the poorest or most marginalized); this would imply that our measured results are an underestimate of the true impact of the program. After conversations with the NGO staff and community stakeholders, we believe it is unlikely that the committees used differential criteria for participation in the two cohorts.

As stated above, propensity score analysis matches youth in two different groups based on 36 observable demographic characteristics, or covariates, Z. Youth are matched based on these demographic characteristics, including: age, sex, education level, number of children, number of dependents, rural or urban, whether or not their father is alive, the number of people living in their house, whether or not the respondent has ever participated in training, and the number of people who earn income in the household. For example, we might match a 19 year old with no apprentice experience on Cohort 1 to a 19 year old in Cohort 2 who also has no apprentice experience. In addition to this demographic information, we were able to match on variables that capture more

subjective aspects of individual character including the participants' values (for example, do you value having your children educated?), life skills (for example, do you set goals for yourself?) and family social supports (for example, are adults able to help you in practical ways?). This matching process offers a way of correcting for the effects of selection bias based on these available demographic, character and social support covariates and provides a more rigorous estimation of the average impact of the program.

After matching the youth, the graph of the area of 'common support' shows the region of comparable youth observations with similar characteristics across the two cohorts. Figure 3.2 shows the histogram of common support across various propensity scores using the Epanechnikov kernel method.[5] Those youth that did not match, or are not in the 'area of common support', are those who are too dissimilar to be comparable (for example, they may be an outlier in that they have too much work experience). Of the 434 youth in this sample, only 24-37 did not match well and were dropped from the analysis (depending on the matching method) because they failed to meet this condition. It is not particularly surprising that so few observations were dropped, given that youth in each of the two cohorts were selected for the program based on the same criteria. The fact that the sample populations are so similar helps to reduce some of the possible bias that is usually introduced with a comparison group. After the matches are made, the difference in means of each survey question for the two groups, weighted by the propensity of treatment, is tested for statistical significance. The average treatment effect on the treated (ATT) tells us the size of the impact of the program, while reducing bias through the matching process. The ATT tells us the estimated difference in the means, given that the person participated in the program. We also calculate the percent change as the difference of the average treatment effect on those who received programming compared to the mean of that outcome variable for all youth in the matched sample.

Finally, linear regression provides another method for analyzing the impact of the program on youth's employment and financial skill outcomes using ordinary least squares (OLS) with and without individual fixed effects. This secondary analytical method

[5] We use PSMATCH2 in STATA, which was developed by Leuven and Sianesi (2003)

serves as a robustness check of the findings from the ATT using propensity score analysis. In the cross-cohort linear regression, we compare estimates the impact of the treatment using the sample of Cohort 1 post-program scores with the Cohort 2 pre-program scores. This comparison aligns with the sample using the propensity score matching because we are comparing the first cohort (those who have completed the program) to the second (those who are entering the program).[6] We estimate the effect in the following OLS cross-cohort linear regression:

$$Y_i = \beta_0 + \beta_1 w_i + \epsilon_i \tag{3.4.1}$$

We use OLS estimation with fixed effects because of the advantage the time-invariant characteristics of youth are absorbed in the individual fixed effect. This removes the influence of these unobservable time-invariant factors, allowing for a less biased estimate of the true impact of the program.

Then we estimate the effect of the program by comparing an individual to himself, before and after the program. In the equation below, Y_{it} represents the employment and financial skill variables from the survey at time, t. The variable w_{it} represents the impact of the time of data collection, indicating whether the survey response is from before or after the program. The coefficient of interest is β_1, which is the effect of time on individual outcomes. This is, the estimate of the average program impact.[7] The individual fixed effect, α_i, captures all the time in-variant individual characteristics that affect the outcome, Y.

$$Y_{it} = \beta_0 + \beta_1 w_{it} + \alpha_i + \epsilon_{it} \tag{3.4.2}$$

[6] A key difference in the two methods is that PSM excludes the 24-37 individuals whose propensity for treatment was outside the area of common support. Linear regression does not exclude these observations.

[7] While the observed effect of the program, β_1, captures the true effect of the program, it also captures the effect of time-variant characteristics, such as maturity.

While an OLS regression, even with controls for individual fixed effects, still cannot account for all unobservable factors affecting program outcomes, the results of the second estimation technique signal the robustness of the propensity score analysis results.

3.5 Results

This section of the paper is organized as follows. First, we discuss the sample of youth participants and provide the descriptive statistics. Second, findings from simple non-parametric tests are presented as a preliminary step in the analysis. Third, the results from propensity score analysis using Epanechnikov kernel matching are discussed. Finally, linear regression results provide a robustness check of the propensity score analysis findings.

3.5.1 Descriptive Statistics

Table 3.5 provides descriptive statistics and balance of youth participating in Cohort 1 and Cohort 2 of the entrepreneurship training program. This table displays observable demographic characteristics, values, social support and life characteristics before and after the matching process. In both cohorts, there are slightly more male youth in the program than female. The participants range in age from 14 to 26 years old, with the average age of 20 years old. Most of the participants are located in rural areas and have an average of seven people living in their household. Most youth live in a male-headed household and a small percent of youth are the head of the household themselves. Many of the youth are single and without children, however more women are married and have children than men in the program. Overall, 16-18 percent of all youth report that their mother is deceased and 28-39 percent report that their father is deceased.

At the start of the program, the majority (58 percent) of youth have completed only primary school (Standard 7). In Cohort 1, only 26 percent of youth completed Form 4 and in Cohort 2, 32 percent completed Form 4 (final level of secondary school). At the start of the program, only a small percentage of participants reported that they

were currently employed (12 percent) and few had previously participated in vocational or skills training or internships. Additionally, only a handful of youth had previously started their own enterprises. Most of the participants live in households where someone earns income, but, of these, the majority come from single income households. A very small percentage of the youth reported that they currently have a savings account and of those who do, most have an individual account. Only a handful of youth reported having applied for a loan in the past and of those who have received the loan, they used it for small business activities, school fees, selling fish, and to upgrade farming activities.

3.5.2 Propensity Score Analysis

Turning to the propensity score analysis, the findings in Table 3.1 suggest that there was a significant impact of the program on youth in Cohort 1 (those who have completed the program) in nine of the survey questions regarding employment and finances using Epanechnikov kernel matching. For the questions about how easy youth think it will be to find employment at the end of the program, their desire to be self-employed, the importance of saving money and how much they are expected to share money with others, there is essentially no difference in how participating youth and non-participating youth responded. Further, there was no difference between participants and non-participants in their belief that the knowledge and skills learned in the program will help them find employment and improve their earnings. This finding suggests that youth enter the program optimistic about their employment and earnings prospects and maintain this belief following completion of the program.

Youth that have completed the program reported that they are significantly more knowledgeable about finding employment in their community compared to those youth who have not yet completed the program (from a starting average of 2.2 to an average of 3.0 on the four point scale). Youth who completed the program not only have more knowledge about finding employment, but reported being substantially more knowledgeable about developing a business plan than youth in the comparison group (from 2.0 to 2.9). Similarly, participating youth who completed the program reported more confidence that they have skills desired by future employers (2.3 to 3.3). According to

these findings, youth not only increased their knowledge about finding employment and creating a business plan as a result of the program, but in their confidence that they have employable skills.

With respect to financial literacy, youth who completed the program reported having 45.5 percent more knowledge about how to create a personal budget and 50.2 percent more knowledge about tracking expenses than youth in the comparison group. Further, youth who completed the program reported that they have 16.5 percent more financial decision-making power in their households and 97.0 percent more savings knowledge. Youth who finished the program are more than twice as likely to attribute their learning about savings from the Mavuno group savings. As mentioned above, the Mavuno savings and lending groups are a key component of the Swisscontact U-Learn program and this experience appears to have made a substantial impact on participants. These findings show significant increases in the financial literacy of marginalized Tanzanian youth who have completed the nine-month U-Learn entrepreneurship-training program.

3.5.3 Linear Regression Results

Table 3.2 presents findings from a linear regression estimating the impact of the program, displaying the results from the raw correlation coefficient and two models. The cross-cohort linear regression model estimates the impact of the treatment using the sample of Cohort 1 post-program scores with the Cohort 2 pre-program scores. This comparison aligns with the sample using the propensity score matching because we are comparing the first cohort (those who have completed the program) to the second (those who are entering the program). For the correlation coefficient and the individual fixed effects model, we pooled the pre-program and post-program data from both cohorts to examine the program impact in a temporal way. The correlation coefficient shows the relationship between each outcome variable and the effect of treatment (without an individual fixed effect) for both cohorts.[8]

In the fixed effects model, we measure the impact of the program on each participant,

[8]This is the equivalent of the estimation in equation 3.4.2 without the fixed effect α_{it}.

controlling for an individual fixed effect as shown in equation 3.4.2. This model accounts for the effects of each individual, including time-invariant characteristics, both observed and unobserved. Although this model implies a parametric form (linear), unlike propensity score analysis, the fixed effect model has the benefit that it accounts for time invariant unobserved individual characteristics.

The linear regression findings suggest that, holding other things constant, the program has a significantly positive impact on youth's knowledge about employment and financial literacy. The cross-cohort linear regression model shows large and positive impacts on employment knowledge and confidence, knowledge about business planning, personal accounting, and on learning how to save. These are the kind of skills, behaviors and attitudes that can affect pathways for future employment and entrepreneurial success. The pooled individual fixed effects model shows large and positive effects on these same outcomes, although generally smaller effect sizes. This may be the case because the pooled individual fixed effects model is controlling for time-invariant individual characteristics, such as ability, that we are not able to account for in the cross-cohort linear regression.

In adherence to the puzzling literature on training program evaluations, some of the linear regression results leave us without consistent conclusions. For example, we observe a positive and statistically significant impact on participants' belief that they have skills employers seek across both models. Yet at the same time, youth are skeptical that the knowledge and skills they learned in the program will help them find employment. In fact, in the cross-cohort regression, the estimate of the program impact on this outcome is actually negative, while it is positive in the fixed effects model. It also may the case that the youth are confident in their skills, but skeptical of the labor environment. This may be indicative of other major hurdles youth face in the structural labor market including corruption, lack of mobility and limited opportunities in their village.

Contrary to the findings from propensity score analysis, the fixed effects model shows a positive effect of the program on the expectation to share most of their income with others. Meanwhile, the propensity score analysis and the cross-cohort regression results show an increase in a related outcome: post-program, youth reported more autonomy

over their finances. However, the fixed effects results did not find the program to have a significant impact on this particular variable. Due to these contradictory findings, no conclusions can be made regarding the program's impact on youth's financial autonomy or the expectation to share their income. The consistency in propensity score analysis and the regression results allows us to conclude that there is a large and significant improvement in participants' employment knowledge and financial literacy. The largest effect of the program was on learning to save. This finding reiterates the focus of the U-Learn program in improving the savings knowledge and behavior of participants. Not only do the Mavuno saving and lending groups appear to have an important impact on the participants' impression of the program, but youth's knowledge about how to apply for a savings account significantly increased over the course of the program as well. Across the nonparametric tests, propensity score analysis, and linear regression results, we observe of large and positive impacts on employment knowledge and confidence, knowledge about business planning, personal accounting, and savings knowledge. The consistency of the results under the different models shows that the program effect is not sensitive to the methodology and provides evidence of a successful educational program.[9]

3.5.4 Discussion

This study answers the research question: How did the U-Learn entrepreneurship training program affect youth knowledge, skills, and attitudes? The findings indicate that the U-Learn entrepreneurship training program in Tanzania increased participants' self-reported knowledge in statistically significant and meaningful ways.

Beyond the substantive finding about training programs, the present study reinforces the use of propensity score analysis using successive cohorts of youth participants as a workable means of establishing a comparison group. Because the same community members governing the U-Learn program selected both cohorts under the same criteria,

[9]It is also important to note that these self-reported outcomes are measured immediately following participation in the program. The estimates of the impact of the program within the time-frame of the study do not ensure that the impact will continue for the participant's lifetime.

much of the possible unobservable biases that could be introduced using the second cohort as a comparison group were avoided. Governments and international development organizations are increasingly expressing a commitment to emphasize evidence-based practice, yet observe the weakness of available evidence. This paper builds on the non-randomized evaluation literature to promote rigorous examination of the effects of social programs using multiple analysis techniques, including propensity score analysis, and a sensitivity analysis to confirm robustness of the results. While RCTs are the gold standard in causal impact evaluation, less disruptive methodologies such as propensity score analysis provide insight into the effects of entrepreneurship training on youth without undermining local authority and capacity.

While most of the development literature addresses the impact of training programs on income-related measurements, such as asset-accumulation or employment status, this program presents evidence on the intermediary effects of training programs. In particular, this study focuses on the attitudes and perceptions of the participants, rather than their direct employment. These measurements are more subjective; for example, students reported whether they think it will be easier to find a job. However, this information provides insight into the mechanisms of employment trajectories, which is of particular relevance for short-term training policy. Evaluations that focus simply on the final income-related measurements of the program may overlook the pathways in which youth experience positive effects of the program. Our results build on business knowledge changes observed in the Karlan and Valdivia (2011) training program evaluation. In both studies, it is possible that employment and income affects may reveal themselves years after the period of evaluation. This study seeks to gain insight into the steps towards employment or small business ownership that were enabled by the program's human capital improvement.

The findings offer support to governments, NGOs and policy makers around the globe who encourage entrepreneurship programming as a way of tackling youth unemployment. The U-Learn program illustrates that training policies for marginalized young people in Tanzania can be effective in helping youth develop the knowledge and skills they will need to improve their livelihoods. Such findings must be kept in perspective, however. A variety of factors external to individuals' knowledge, skills, and attitudes

may affect their ability to find or create employment opportunities. Even the best training cannot overcome lack of capital, exclusion from financial services, pervasive corruption, and social prejudice that youth face as they enter the labor market. Further, while we show the effect of the program on youth's attitudes, skills and knowledge, the short time-frame of the study does not allow us to measure the impact of the program on long-term employment or income. And while results suggest that training can have a positive impact on youth's knowledge and skills, the utilization of that training will depend on a wider variety of social policy and supportive conditions being in place. Regardless, identifying successful ways to enable youth to improve their livelihoods not only alleviates immediate poverty, but gives meaning to the lives of young people through confidence, occupation and independence.

3.6 Figures

Figure 3.1: Timeline of Data Collection of the Two Participating Youth Cohorts

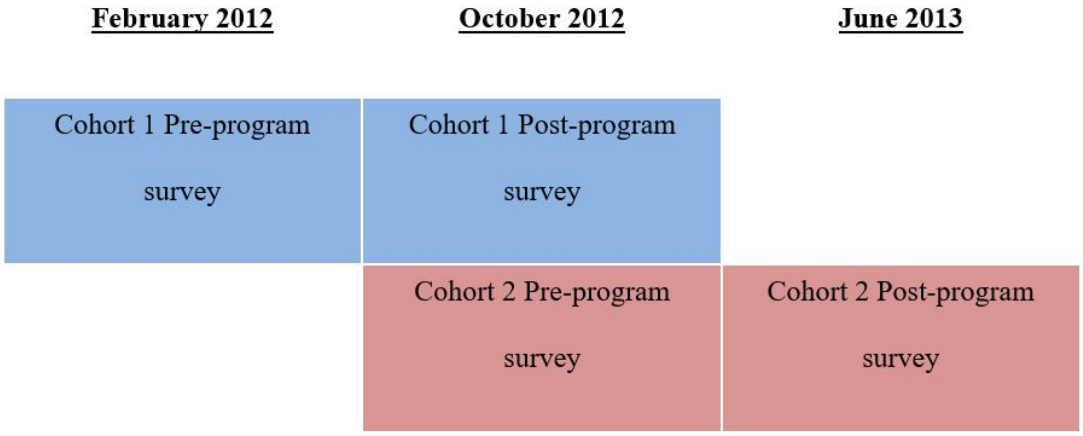


Figure 3.2: Area of Common Support for Propensity Score Matching

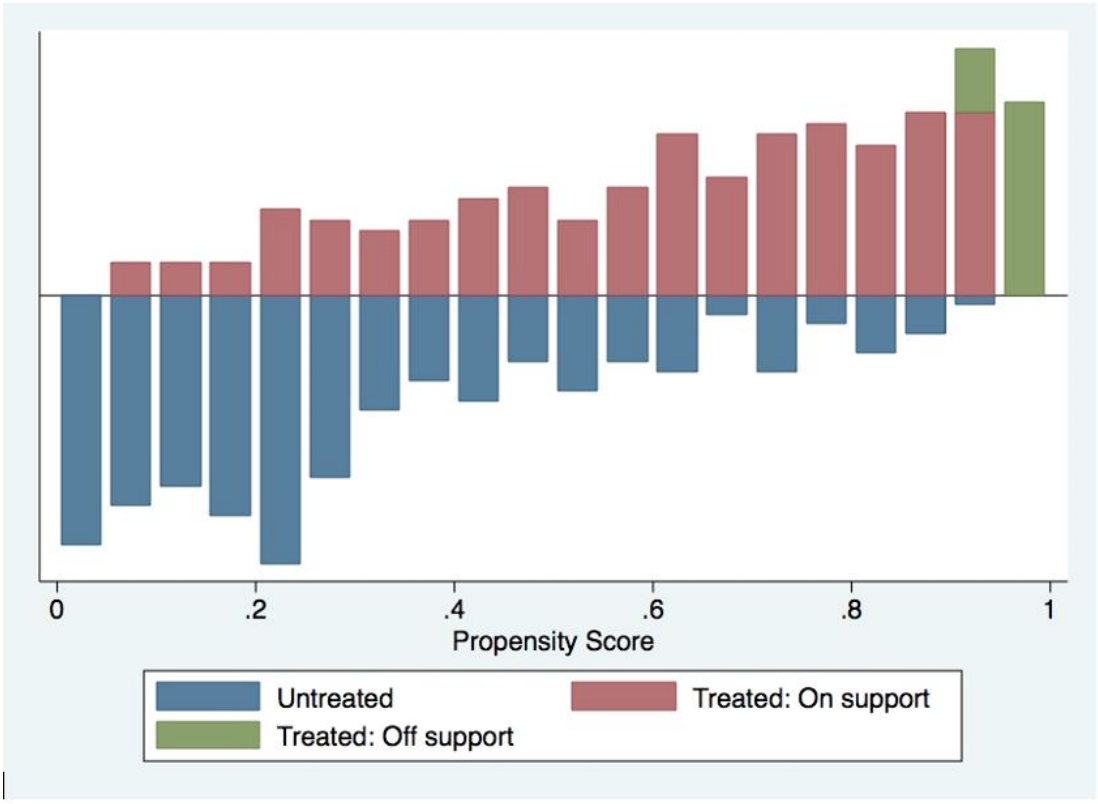


Table 3.1: Propensity Score Analysis Findings using Epanechnikov Kernel Matching

Variable	ATT (Standard Error)	Percent Change
Do you know how to find employment in your community?	1.04*** (0.14)	55.32%
Do you know how to develop a business plan?	1.21*** (0.13)	70.35%
How easy do you think it will be to find employment at the end of this program?	0.12 (0.16)	4.12%
Would you like to be self-employed?	0.18 (0.13)	5.03%
Will the knowledge and skills you learn in this program help you find employment?	-0.13 (0.11)	3.59%
Will the knowledge and skills you learn in this program help improve your earnings?	0.02 (0.18)	0.57%
Do you have skills that employers are looking for?	1.19*** (0.25)	61.66%
Do you know how to create a personal budget?	0.97*** (0.18)	45.54%
How much do you know about tracking your expenses?	1.05*** (0.22)	50.24%
When you have money, are you able to decide how to use it?	0.45*** (0.21)	16.54%
Do you know how to apply for a savings account?	1.27*** (0.21)	96.95%
How important is it to you to save money?	-0.07 (0.13)	1.90%
Has group savings helped you to learn to save (on your own)?	1.83*** (0.27)	108.28%
How comfortable do you feel borrowing money from a savings or credit institution?	0.98*** (0.28)	58.68%
When you have money, are you expected to share most of your money with others?	-0.06 (0.15)	2.46%
Observations	434	

* p<0.10, ** p<0.05, *** p<0.01

Table 3.2: Linear Regression Program Impact Estimation

Outcome Variable	Cross-cohort Linear Regression (SE)	Correlation Coefficient (SE)	Pooled Individual Fixed Effects Model (SE)
Do you know how to find employment in your community?	1.002***	0.935***	0.942***
	(0.083)	(0.054)	(0.053)
Do you know how to develop a business plan?	1.115***	1.014***	1.012***
	(0.093)	(0.055)	(0.053)
How easy do you think it will be to find employment at the end of this program?	0.202**	0.292***	0.294***
	(0.086)	(0.053)	(0.048)
Would you like to be self-employed?	0.214***	0.071	0.071*
	(0.080)	(0.049)	(0.042)
Will the knowledge and skills you learn in this program help you find employment?	-0.139**	0.059	0.061*
	(0.063)	(0.040)	(0.038)
Will the knowledge and skills you learn in this program help improve your earnings?	0.007	0.108**	0.108***
	(0.065)	(0.042)	(0.035)
Do you have skills that employers are looking for?	1.267***	1.155***	1.150***
	(0.088)	(0.057)	(0.057)
Do you know how to create a personal budget?	1.019***	0.844***	0.844***
	(0.097)	(0.060)	(0.058)
How much do you know about tracking your expenses?	1.132***	0.867***	0.871***
	(0.098)	(0.060)	(0.057)
When you have money, are you able to decide how to use it?	0.498***	0.083	0.083
	(0.103)	(0.062)	(0.062)
Do you know how to apply for a savings account?	1.043***	1.161***	1.162***
	(0.099)	(0.064)	(0.058)
How important is it to you to save money?	-0.031	0.108**	0.104***
	(0.056)	(0.042)	(0.039)
Has group savings helped you to learn to save (on your own)?	1.791***	1.641***	1.640***
	(0.096)	(0.060)	(0.060)
How comfortable do you feel borrowing money from a savings or credit institution?	0.874***	0.862***	0.862***
	(0.103)	(0.070)	(0.061)
When you have money, are you expected to share most of your money with others?	0.071	0.327***	0.330***
	(0.099)	(0.060)	(0.055)
Observations	434	868	868

* p<0.10, ** p<0.05, *** p<0.01

Table 3.3: Questionnaire Response Options

Survey Question	Response Options
Do you know how to find employment in your community?	1 I know almost nothing
	2 I know a little
	3 I know some things
	4 I know a lot
Do you know how to develop a business plan?	1 I know almost nothing
	2 I know a little
	3 I know some things
	4 I know a lot
How easy do you think it will be to find employment at the end of this program?	1 Not at all
	2 A little
	3 Somewhat easy
	4 Very easy
Would you like to be self-employed?	1 Not at all
	2 A little
	3 Somewhat
	4 Very much
Will the knowledge and skills you learn in this program help you find employment?	1 Not at all
	2 A little
	3 Somewhat
	4 A great deal
Will the knowledge and skills you learn in this program help improve your earnings?	1 Not at all
	2 A little
	3 Somewhat
Do you have skills that employers are looking for?	1 Not at all
	2 A little
	3 Somewhat
	4 Many
Do you know how to create a personal budget?	1 I know almost nothing
	2 I know a little
	3 I know some things
	4 I know a lot
How much do you know about tracking your expenses?	1 I know almost nothing
	2 I know a little
	3 I know some things
	4 I know a lot

Table 3.4: Propensity Score Matching Sensitivity Analysis

	Near neighbor replacement	Near neighbor no replacement	10 nearest neighbors	Radius matching	Kernel matching	Epanechnikov kernel matching	Mahalanobis matching
Do you know how to find employment in your community?	0.94*** (0.14)	1.03*** (0.08)	0.99*** (0.11)	1.11*** (0.17)	1.06*** (0.19)	1.04*** (0.14)	0.91*** (0.15)
Do you know how to develop a business plan?	1.16*** (0.15)	1.19*** (0.09)	1.17*** (0.11)	1.25*** (0.19)	1.22*** (0.17)	1.21*** (0.13)	1.17*** (0.14)
How easy do you think it will be to find employment at the end of this program/school?	0.18 (0.13)	0.18** (0.08)	0.14 (0.11)	0.22* (0.14)	0.11 (0.12)	0.12 (0.16)	0.14 (0.14)
Would you like to be self-employed?	0.11 (0.13)	0.15** (0.07)	0.18 (0.10)	0.28** (0.17)	0.17 (0.16)	0.18 (0.13)	0.10 (0.09)
Will the knowledge and skills you learn in this program/school help you find employment?	-0.20 (0.09)	-0.15** (0.06)	-0.17** (0.08)	-0.11*** (0.14)	-0.14* (0.10)	-0.13 (0.11)	-0.11*** (0.12)
Will the knowledge and skills you learn in this program/school help you improve your earnings?	0.09 (0.11)	-0.02 (0.06)	0.06 (0.08)	-0.01 (0.12)	0.01 (0.13)	0.02 (0.18)	-0.03 (0.12)
Do you have skills that employers are looking for?	1.09*** (0.15)	1.29*** (0.08)	1.21*** (0.12)	1.02*** (0.26)	1.24*** (0.23)	1.19*** (0.25)	1.00*** (0.22)
Do you know how to create a personal budget?	1.02*** (0.18)	1.06*** (0.09)	1.01*** (0.13)	0.94*** (0.23)	1.00*** (0.18)	0.97*** (0.18)	0.81*** (0.29)
How much do you know about tracking your expenses?	1.04*** (0.17)	1.17*** (0.09)	1.07*** (0.13)	0.98*** (0.27)	1.07*** (0.23)	1.05*** (0.22)	0.86*** (0.23)
When you have money, are you able to decide how to use it?	0.62*** (0.18)	0.51*** (0.09)	0.58*** (0.14)	0.45*** (0.30)	0.47*** (0.18)	0.45*** (0.21)	0.44*** (0.16)
Do you know how to apply for a savings account?	1.43*** (0.14)	1.26*** (0.10)	1.26*** (0.12)	1.25*** (0.27)	1.27*** (0.19)	1.27*** (0.21)	1.12*** (0.19)

Table 3.5: Covariate Matching Results for Epanechnikov PSM Estimator

Variable	Sample	Mean		t-Test	
		Treated (n=202)	Control (n=232)	t-statistic	p-value
Female	Unmatched	0.424	0.476	-1.100	0.273
	Matched	0.418	0.440	-0.420	0.674
Age	Unmatched	20.33	20.42	-0.330	0.743
	Matched	20.30	20.50	-0.750	0.453
Married	Unmatched	0.182	0.104	2.350	0.019
	Matched	0.163	0.146	0.460	0.649
Have children	Unmatched	0.448	0.177	3.930	0.000
	Matched	0.380	0.429	-0.510	0.609
Have dependents	Unmatched	1.813	0.961	5.080	0.000
	Matched	1.794	1.608	0.910	0.361
Rural	Unmatched	0.724	0.407	6.980	0.000
	Matched	0.701	0.693	0.170	0.868
Mother is alive	Unmatched	0.842	0.823	0.550	0.582
	Matched	0.842	0.845	-0.0600	0.952
Father is alive	Unmatched	0.616	0.723	-2.380	0.018
	Matched	0.641	0.598	0.860	0.392
Last grade completed	Unmatched	9.098	8.866	0.490	0.626
	Matched	9.098	9.045	0.0900	0.924
Number of people living in household	Unmatched	7.251	6.554	2.360	0.019
	Matched	7.098	6.994	0.310	0.755
Youth previously participated in vocational or skills training before entering the program	Unmatched	0.0985	0.165	-2.020	0.044
	Matched	0.103	0.113	-0.300	0.767
Youth were employed at the start of the program	Unmatched	0.124	0.117	0.220	0.827
	Matched	0.114	0.108	0.180	0.859
Youth had participated in an internship before entering the program	Unmatched	0.222	0.286	-1.530	0.128

Variable	Sample	Mean		t-Test	
		Treated (n=202)	Control (n=232)	t-statistic	p-value
Youth had participated in an internship before entering the program	Unmatched	0.222	0.286	-1.530	0.128
	Matched	0.234	0.267	-0.740	0.463
Number of people earning income in the household	Unmatched	1.473	1.684	-1.920	0.056
	Matched	1.500	1.579	-0.650	0.515
Youth entered the program with a savings account	Unmatched	0.0591	0.0693	-0.430	0.668
	Matched	0.0652	0.0573	0.310	0.754
Youth had applied for a loan before entering the program	Unmatched	0.0345	0.0390	-0.250	0.805
	Matched	0.0326	0.0217	0.640	0.521
Before making a decision about spending money, do you consider the options?	Unmatched	3.232	3	2.650	0.008
	Matched	3.207	3.264	-0.610	0.541
Do you think making good decisions can improve your life?	Unmatched	3.665	3.656	0.140	0.889
	Matched	3.685	3.742	-0.960	0.340
Are you willing to speak up for your ideas when a friend disagrees with you?	Unmatched	3.626	3.470	2.210	0.027
	Matched	3.625	3.704	-1.180	0.241
When something you try fails, do you try again?	Unmatched	3.222	3.260	-0.480	0.635
	Matched	3.239	3.340	-1.130	0.260
Are you confident in your work skills?	Unmatched	2.902	2.433	3.880	0.000
	Matched	2.853	2.727	0.940	0.347
Do you set goals for yourself?	Unmatched	3.389	3.052	3.710	0.000
	Matched	3.348	3.480	-1.490	0.137
Do you take action to achieve these goals?	Unmatched	3.054	2.661	4.380	0.000
	Matched	3.011	3.123	-1.180	0.237
Has your life improved because you have made good decisions?	Unmatched	2.823	2.584	2.810	0.005
	Matched	2.810	2.851	-0.460	0.648
How important is it to you to get additional training or education after completing this program?	Unmatched	3.749	3.429	4.790	0.000
	Matched	3.728	3.709	0.310	0.757
Are people your age (peers) willing to listen when you are having problems?	Unmatched	2.916	2.857	0.750	0.454
	Matched	2.913	2.830	0.930	0.352
Are adults you know willing to help you in practical ways (loan money, meals, or clothes)?	Unmatched	2.759	2.389	4.370	0.000
	Matched	2.712	2.716	-0.0500	0.960
Are adults you know available when you need them?	Unmatched	3.044	2.652	4.770	0.000
	Matched	2.967	3.003	-0.400	0.692
Do you value being employed?	Unmatched	3.709	3.596	1.790	0.074
	Matched	3.717	3.778	-1.040	0.299
Do you value owning your own business?	Unmatched	3.700	3.465	3.270	0.001
	Matched	3.690	3.734	-0.700	0.486
Observations	Unmatched	226	235		
	Matched	202	232		

Chapter 4

Favoritism and Farming: Agricultural Productivity and Wife Order in Polygynous Households

4.1 Introduction

Women play a key role in Sub-Saharan African food production, where they make up the majority of small-scale farmers and produce 60 to 70 percent of the food supply (Gawaya, 2008). This large contribution to agricultural production is viewed as a contributing source of the persistence of polygyny (Jacoby, 1995). Gendered determinants of intra-marriage bargaining power, such as education, large age differentials, and lack of access to credit may have a negative effect on women's agricultural productivity. Polygyny can alter the bargaining power structure within the household through the additional resource competition, or improved cooperation, or both. However, it is not yet clear what effect polygyny has on the distribution of agricultural resources across plots managed by men and women in the same household. I explore the effect of the number of wives, wife order and joint plot management on agricultural productivity for

farming households in Tanzania.[1]

This paper builds on a well-established body of literature that examines the sources of agricultural productivity differences between men and women. Led by the seminal work of Udry (1996), a number of studies (Quisumbing and Maluccio, 2003; Akresh, 2005; Rangel and Thomas, 2012) test for Pareto efficiency through comparisons of agricultural yield differences across plots within households. Efforts to achieve efficiency may be complicated by the household structure and the family heirarchy of polygyny. Akresh, Chen, and Moore (2011) find that, in Burkina Faso, polygynous households have lower yield differences between spouses and thus are more productively efficient than monogamous households. This result stands in contrast to the findings by Peterman et al. (2011); they conclude that the differences in agricultural productivity between men and women in Uganda and Nigeria are, in fact, driven by the less efficient polygynous households in both countries. Dauphin (2013) finds mixed evidence of the effect of polygyny on yield in Benin, Burkina Faso, and Senegal. Her explanation is that the influence of polygyny on intra-household efficiency is subject to cultural context, which is often determined by tribe, geography and local norms. While these papers provide an excellent starting point for an examination of agricultural productivity differences between monogamous and polygynous households, it is not yet clear what effect wife order has on allocative efficiency across plots that are jointly managed in polygynous households.

Cooperation among co-wives in a polygynous household would be most efficient; however, the anthropological evidence indicates that co-wife relationships within polygynous households in Sub-Saharan Africa are often characterized by negative competition and conflict. Jankowiak, Sudakov, and Wilreker (2005) and Kazianga and Klonner (2009) find evidence of competition and an unequal distribution of wealth across co-wives. Rates of polygyny are declining in Tanzania, but the practice is still common. According to the Social Institutions and Gender Index (SIGI) of the OECD, 23 percent of Tanzanian women were in polygynous marriages in the 2004-2005 Demographic and Health Surveys (DHS), a drop from 29 percent during the 1990s. To better understand

[1] This chapter is co-authored with Amy L. Damon and Vincent Siegerink.

the role of polygyny in household production in Tanzania, I measure its effect on agricultural allocative efficiency across plots, and as a function of each person's position in the household.

Extremely few plots are managed solely by women, thus this analysis compares the effect of polygyny on jointly managed plots (jointly managed by the husband and at least one wife) versus male-only managed plots. Plot management in the sample was determined by the survey question "who decided what to plant on this plot?" Forty-eight percent of crops are managed jointly by husbands and wives. Although ownership and decision-making may be subject to reporting bias, these measurements are commonly used in the literature. In this sample, the average number of wives in married households is 1.22 in 2009, 1.23 in 2011 and 1.25 in 2013. I examine the effect of resident wives (wives living in the sampled household) in the of polygyny, which limits the number of wives in this sample to two.[2] Additionally, most second wives that manage plots, do so with the first wife as well (89 percent). However, for households that have two wives, only eight percent of the first wife's jointly managed plots are also jointly managed with the second wife.

Although there is a large literature on the effect of polygyny on agricultural productivity in West Africa, I expand this test for efficiency to the East African context and, to the best of my knowledge, this is the first paper to do so using agricultural data. I also build on the understanding of household intra-household power structures by examining the effect of joint plot management by the first and second wife, exploiting differences in bargaining power and seniority within the household.

[2]Fifty three percent of polygynous households in the sample include all of the husband's wives (i.e. do not have non-resident wives). Thus, in about half of all polygynous households, a husband has other wives who are not counted in this analysis. The definition of a household, according to LSMS-ISA is "people who live together and share income and also basic needs. In other words, residents of a household share the same center of production and consume from that center."

4.2 Conceptual Framework

In the conceptual model, similar to that of Udry (1996), I develop the conditions that should hold if the household is operating plots efficiently. I then test whether the number of wives or joint management in the household has an effect on the efficient allocation of resources. For the sake of understanding the intra-household relationship between men and women, I limit the household size in this model to three: a husband and two wives.

Each person in the household has his/her own utility function:

$$U_h(C_h, C_{w1}, C_{w2}, Z) \tag{4.2.1}$$

$$U_{w1}(C_h, C_{w1}, C_{w2}, Z) \tag{4.2.2}$$

$$U_{w2}(C_h, C_{w1}, C_{w2}, Z) \tag{4.2.3}$$

where:

$$h = husband;\ w_1 = wife\ 1;\ w_2 = wife\ 2 \tag{4.2.4}$$

The arguments of the household utility function are as follows: C is the consumption of private goods and Z is the consumption of a public good. Total labor for each individual, N is fixed and thus does not appear in the utility functions. Consumption and labor supply are indexed specifically for each member of the household. Total consumption of private goods is constrained by:

$$C = C_h + C_{w1} + C_{w2} \tag{4.2.5}$$

Public good production (i.e. cooked meals or a clean house) within the household is determined by the labor allocated from each household member:

$$Z = Z(N_{w1}^Z, N_{w2}^Z, N_h^Z) \tag{4.2.6}$$

Production (Y) of crop k in the household is defined as:

$$Y^k = \Sigma_{i \epsilon P^k} B^k(N^i_{w1}, N^i_{w2}, N^i_h, A^i, T^i) \tag{4.2.7}$$

Here, i is an index for different plots of land controlled by the household, for production of some crop, k; P^k denotes the set of plots on which k is grown. N indicates husband and wives' labor applied to plot i, A is the land area of plot i, and T is the amount of inputs (e.g. fertilizer) allocated to plot i. B^k is the production function and is assumed to be concave in all arguments. The technology of production is permitted to vary across crops, but not across plots within a single crop. The restrictions for male and female labor supply are a function of time spent on plots and on household public good production:

$$N_{w1} = N^z_w + \Sigma^I_{i=1} N^i_{w1} \tag{4.2.8}$$

$$N_{w2} = N^z_w + \Sigma^I_{i=1} N^i_{w2} \tag{4.2.9}$$

$$N_h = N^z_h + \Sigma^I_{i=1} N^i_h \tag{4.2.10}$$

This restriction implies that household labor can be allocated to either production of the household public goods or to farming across all plots, i. Without leisure in the model, the total amount of labor allocation for each household member is fixed. This simplification implies that individuals choose between allocating time to the household public good or to plots. Similar to Udry (1996), this framework assumes that there are no labor or land markets.

The maximization problem is also subject to the household budget constraint:

$$p \cdot C \leq p \cdot Y \tag{4.2.11}$$

A cooperative household, which by definition is efficient, would have a kind of sharing rule that is based on a household utility function that is a weighted average of the utilities of the three members. Thus, the household needs to choose C_{w1}, C_{w2}, C_h; N_{w1},

N_{w2}, N_h for each plot; and N^z_{w1}, N^z_{w2}, N^z_h, to maximize:

$$U = \mu_1 U_h(C_h, C_{w1}, C_{w2}, Z, N_h, N_{w1}, N_{w2}) + \mu_2 U_{w1}(C_h, C_{w1}, C_{w2}, Z, N_h, N_{w1}, N_{w2}) + \mu_3 U_{w2}(C_h, C_{w1}, C_{w2}, Z, N_h, N_{w1}, N_{w2}) \quad (4.2.12)$$

$$\mu_1 + \mu_2 + \mu_3 = 1 \quad (4.2.13)$$

Maximization of this household utility function (4.2) is subject to the budget constraint (4.2.11), production technology (4.2.7), household labor conditions (4.2.8)-(4.2.10) and technology for producing Z public goods (4.2.6). Equation 4.2.13 is a normalization. There is no leisure in this model. If the household is operating efficiently, this maximization problem implies that the household would choose the same allocation of inputs over these plots as the production maximization problem (maximizing the crop production function, B^k, subject to optimal aggregate labor allocations to each crop). In other words, conditional on optimal amounts of inputs and of each of the three types of labor to each crop, efficiency implies that household utility is maximized if labor and agricultural inputs are allocated across plots in which crop k is grown in the way that also maximizes production of that crop.

Additionally, if I assume that B^k is increasing in all arguments, and strictly increasing in land area, A^i, then, under Pareto efficiency, two plots of the same size and characteristics should yield the same output, regardless of the gender of the cultivator. This leads to the main testable hypothesis:

Hypothesis 1 Conditional on inputs, if the cooperative household model holds, the gender and wife order of the plot managers (and thus, whether the plot is jointly managed by the first wife or the second wife) should not be a significant predictor of yield.

The standard separation result of an agricultural household model should hold, where production decisions are independent of preferences within the household.[3] Any differences in yield based on whether the plot is jointly managed will thus be evident of

[3]This assumes that hired labor is a perfect substitute for household labor.

intra-household allocative inefficiency.

4.3 Empirical Approach

This paper uses the Tanzania National Panel Survey (TZNPS), which is one of the LSMS Integrated Surveys on Agriculture (LSMS-ISA), to examine households' distribution of agricultural inputs across plots in polygynous and monogamous households. Using the three waves of this household survey, 2008-2009, 2011 and 2013, allows the ability to examine the effect of longitudinal household changes in marital status, such as entering into a monogamous marriage or gaining an additional wife.

The TZNPS was initiated to provide comprehensive high-quality household-level data to the government of Tanzania, with the aim of evaluating policy initiatives to alleviate poverty. The nationally representative survey includes a wide range of information on household characteristics, including family composition, labor, health and education. The agricultural data include land characteristics, outputs and inputs, separately for each plot. The data were collected for each crop planted on a specific plot. Information was also collected about the management of the plot (who decides what to plant), as well as crop-level information such as the quantity harvested and the area (acreage) on which that crop was harvested. Multiple crops are often harvested on a single plot.[4] When the data have been matched across waves, plots and crops, the sample includes 891 households, 1283 plots and 1603 crop-level observations. The main outcomes of interest are yield (measured as kilograms per acre), fertilizer used per acre (kilograms per acre), labor applied per acre (person-days of labor) and total crop value (using farmer-estimated prices for all crops). The outcome variables are logarithmically-transformed using the inverse hyperbolic sine function to adjust for a large number of zeros.

As indicated above, the assumption of household production efficiency implies that variations in yield across plots or crops should be explained entirely by plot or crop

[4]Our analysis takes place on the crop level. While some of the agricultural variables are recorded at the crop level, many are recorded at the plot level. So in most cases I apply plot-level characteristics to crop-level observations. This implies the assumption that inputs are allocated equally across crops on the same plot.

characteristics. I examine whether this holds by evaluating the difference in yields between joint and husband-only plots. Here I examine equal yield for plots (i), defining yield per unit of land as $Q^k(A^i)$:

$$Q^k(A^i) = (B^k[N^i_{w1}(A^i), N^i_{w2}(A^i), N^i_h(A^i), T^i(A^i)])/A^i \tag{4.3.1}$$

If the cooperative household model holds, the yield of plot i of household h for crop k should depend only on the plot characteristics and input quantities, not on the number of wives or wife order of the plot manager. Using this definition of yield, I can test whether polygyny, joint management and wife order affect the yield on the plot. In a second test of Pareto efficiency, I test whether polygyny affects the use of agricultural inputs such as labor, fertilizer and pesticide. Differences in agricultural input allocation for joint and husband-only plots would indicate greater or lesser extents of allocative efficiency in polygynous households.

I first estimate the 2009 cross-sectional effect of the number of wives on the yield of crop k in year t for household h on plot i by running the following ordinary least squares regression:

$$Q_{htki} = \beta_1 X_{htki} + \beta_2 G_{htki} + \beta_3 F_{htki} + \epsilon_{htki} \tag{4.3.2}$$

Here, X represents plot characteristics, G is an indication of a plot jointly managed with husband and wife, F represents the number of wives. The effect of the number of wives is captured by the parameter β_3, and β_2 represents the effect of joint plot management on yield, Q. As the final agricultural outcome, yield should capture any allocative inefficiencies across plots, however, I also test for allocative inefficiency using the same regression with fertilizer, labor and total crop value as outcomes. Equation (4.3.2) represents a cross-sectional test of Pareto efficiency within the household. Efficiently producing polygynous households are then given by $(\beta_2 = \beta_3 = 0)$. The possible effects

of polygyny are the following:

$$\beta_3 < 0;\ \text{polygynous households are less efficient} \tag{4.3.3}$$

$$\beta_3 = 0;\ \text{polygyny has no effect} \tag{4.3.4}$$

$$\beta_3 > 0;\ \text{polygynous households are more efficient} \tag{4.3.5}$$

The possible effects of the number of wives and of joint management are suggested by the previously described anthropological and economic accounts of polygynous households. For example, the scenario described in equation (4.3.5) may be explained by greater cooperation between wives in polygynous households (Akresh, Chen, and Moore, 2011). Alternatively, polygyny could be a source of conflict that jeopardizes overall productivity, which would result in equation (4.3.3). It is possible that β_2 and β_3 have opposite signs. If $\beta_2 < 0$ and $\beta_3 > 0$, then the polygyny has a positive effect on yield, but joint management has a negative effect on yield, implying a possible cooperation benefit of additional persons in the household, but an inefficient allocation of agricultural inputs. And if $\beta_2 > 0$ and $\beta_3 < 0$, then polygyny has a negative effect on yield and joint management has a positive effect on yield, implying that additional wives may reduce productivity but jointly managed plots are more efficient.

In a more narrow analysis to understand the effect of wife order in polygyny, I measure the differential effects of the first and the second wife joint management on agricultural productivity with interaction terms. To do this, I estimate the following equation:

$$Q_{htki} = \beta_1 X_{htki} + \beta_2 G_{1htki} + \beta_3 G_{2htki} + \beta_4 F_{htki} + \beta_5 (F_{htki} * G_{1htki}) + \beta_6 (F_{htki} * G_{2htki}) + \epsilon_{htki} \tag{4.3.6}$$

In this estimation, β_2 represents the effect of joint management of the husband with the first wife and β_3 represents the effect of joint management of the husbanda with the second wife.[5] And as previously stated, the analysis of resident wives limits the sample to households with a maximum of two wives. First wives tend to have the most seniority,

[5] As indidcated in the introduction, most plots that are managed by the second wife with husbands are also managed by the first wife. So effectively, this is the effect of joint management between three people.

thus, the effect of joint plot management with a first wife is likely to be different than the effect with a second wife. I expect that the coefficient on joint management for wife 1 (β_2) to be negative for the first wife, implying that joint management would results in lower agricultural input allocations and that this would negatively affect yield. These results would corroborate those of Udry (1996). As an extension of the traditional model, I expect that this term for the second wife, (β_3) to be positive, implying that the involvement of the second wife in joint management would have a positive effect on yield and inputs. In a household with two resident wives, I hypothesize that having the second wife also as a plot manager is an indicator of concentrated effort on production for that plot. Additionally, β_5 and β_6 capture the effect of wife order interacted with jointly-managed plots. Equation (4.3.6) is estimated using 2011 cross-sectional data in Table 4.4 and using 2013 cross-sectional data in Table 4.5. Again, Q_{htki} represents the main agricultural outcomes (maize yield, fertilizer, labor and crop value) for household h, crop k, plot i at time t.

To account for time-invariant characteristics of households, plots and crops that affect agricultural yield, I combine all three years of data and estimate the following regression with fixed effects:

$$Q_{htki} = \beta_1 X_{htki} + \beta_2 G_{1htki} + \beta_3 G_{2htki} + \beta_4 F_{htki} + \beta_5 (F_{htki} * G_{1htki}) + \beta_6 (F_{htki} * G_{2htki}) + \alpha_{hki} + \epsilon_{htki} \tag{4.3.7}$$

where α_{hki} is a fixed effect pertaining to household h for crop k and plot i. This regression is estimated over three waves of data, where t represents year. The fixed effect captures the average household, plot and crop average levels of covariates over the three years, thus X represents the effect the changes in the covariates over the three years of observation.[6] In this model, β_2 and β_3 represent the effect of changes in joint management with the first and second wife in the household. I again anticipate in this fixed effects model that the coefficient on joint management with the first wife would

[6]Fifteen percent of of plot observations are dropped due to changes in the cultivated crops over 2009-2013.

be positive and that the coefficient on joint management with the second wife would be negative. Finally, β_5 and β_6 capture the interaction effect of jointly managed plots and changes in the number wives. Table 4.6 estimates equation (4.3.7) for outcomes maize yield, fertilizer use, labor use and crop value.

In the examination of the effect of a household characteristic, such as number of wives and joint management, on yield, it is important to understand directionality of bias. Polygyny is likely to be correlated with unobserved characteristics of the household such as wealth,[7] preferences for family size and farmer quality that may affect yield (Jacoby, 1995; Akresh et al, 2011). Polygyny has been instrumented in the literature by quantity of land inherited and ethnicity (Akresh et al, 2011; Dauphin, 2013). Ethnicity, although collected in the LSMS-ISA, is not publicly released. Quantity of land inherited was also collected, but only 3 percent of agricultural households answered this question (possibility to due sensitivity issues around land ownership in Tanzania). I did perform the analysis using the ratio of boys to girls in a household and religion as instruments for polygyny, but the instruments often had weak explanatory power that resulted in low F-statistics.

In Tanzania, polygyny is an expensive investment due to bride prices. It is common practice for husbands to pay a bride price (ten to fifty cattle) to the parents of the new wife. Because of this, polygyny is highly correlated with wealth. Although I control for household consumption in all regressions, it is likely that the error term captures unobserved wealth that is positively correlated with yield and agricultural inputs. Wealthier farmers are likely to have better knowledge of cultivation practices and are likely to be more able to afford purchasing agricultural inputs such as hired labor and fertilizer. In this case, the observed coefficient on total wives would over-estimate the effect of polygyny on yield and agricultural inputs. This omitted variable bias would also over-estimate the effect of joint management on yield and agricultural inputs as well. Another possible source of bias is reverse causality. Larger yields may give way to the ability to afford an additional wife. This would also result in an overestimate of the impact of polygyny on yield. Measurement error is the final source of bias in this estimation strategy. Imprecise estimates of land area, amounts of harvested crops,

[7] Although I control for monthly household consumption, wealth is not directly observed.

and prices, in addition to recall bias in use of agricultural inputs may either upwardly or downwardly bias the estimate of the impact of polygyny and joint management on agricultural productivity.

The current analysis includes cross-sectional analysis for multiple years and a household-crop-plot fixed effect regression to measure changes over time. The cross-sectional analysis only controls for observable plot and household characteristics in the estimation of the impact of polygyny on agricultural productivity. However, the fixed effects model accounts for time-invariant unobservable household, plot and crop-level characteristics.

4.4 Descriptive Statistics

I begin the examination of the effect of polygyny on plot input allocation with descriptive statistics of each plot by various changes in marital status between across waves (2009 to 2011 and 2011 to 2013). Table 4.1 shows the variation in outcomes (yield), inputs (fertilizer, farm size, soil quality) and and other characteristics (monthly household consumption and joint management) across changes in marital status. Average fertilizer usage per hectare is significantly different for those households that enter into a monogamous marriage and those that enter into a polygynous marriage. Yields are highest among household that entered into monogamous marriages, lower for those in polygynous marriages and lowest for unmarried household heads (male or female). The household heads that enter into a monogamous marriage between 2009 and 2013 are also much less likely to jointly manage plots with their new spouses relative to polygynous marriages, and this may be a likely factor in the higher yield in monogamous households.

Table 4.2 shows plot and household descriptive statistics over the time period of the panel. The average number of wives in the entire sample is .778 in 2009, .841 in 2011 and .90 in 2013. And as households increase the number of wives (or enter into marriages), they also jointly manage more of their plots. In 2009, 35.5 percent of plots are jointly managed, but by 2013, 52.3 percent of plots are jointly managed. These changes in household dynamics and plot level management over time allow us to measure the effect

of polygyny on agricultural productivity.

4.5 Results and Discussion

In the investigation of the relationship between polygyny and agricultural efficiency, I first examine the effect of total wives and joint management on yield using a 2009 cross-sectional estimation. In Table 4.3, I see that number of wives had no effect on maize yield or crop value. However, polygyny is associated with fewer agricultural inputs. Column (2) shows that the total number of wives in a household has a negative and significant relationship with fertilizer use, while column (3) shows a negative and significant relationship with quantity of labor applied to the crop (measured in person-days). Note that these estimations are cross-sectional and without fixed effects. While the true relationship is causal, the aforementioned possible econometric problems (omitted variables, reverse causality and measurement error) may introduce bias into my estimation of the effect. The later results from the fixed effects estimation are less likely to have such bias.

In this 2009 cross-sectional analysis, I also examine the effect of joint plot management on yield, inputs and crop value. In column (2), joint management of the husband with wife one has a positive and significant (10% level) relationship with fertilizer use and in column (4) joint management of the husband with wife one has a positive and significant relationship with crop value. Joint management with the second wife has a positive and significant relationship with labor, but no significant relationship with any of the other outcomes. Although an increase in the number of wives is associated with less overall fertilizer use, joint management with the first wife is associated with more fertilizer use.

Next, I examine the effect of polygyny and joint management on 2011 yield using another cross-sectional regression. These results are shown in Table 4.4. Between 2009 and 2011, although 92 percent of couples remained with the current number of spouses, six percent of farmers added one wife to the union and 1.5 percent gained two or more wives. In column (1), I observe that polygyny is associated with a statistically significant decrease in maize yield. Although the coefficients on the number of wives are negative for fertilizer

use, labor and crop value, they are not statistically significant.

Joint management with the first wife does not have a strong or consistent effect on agricultural productivity or inputs. And despite large effect sizes for joint management with the second wife, the standard errors of the estimates are also large and thus none of the estimates are statistically significant.

In this estimation, I have added the interaction of joint management with the total number of wives to understand the effect of wife one or two joint management and conjunction with polygyny. The interaction of joint management with the second wife and the total number of wives has a positive and significant relationship with crop value, but the interaction terms generally do not have a significant or consistent effect on the outcomes.

In a further cross-sectional estimation of the effect of polygyny on agriculture, Table 4.5 displays the effect of total wives in 2013 on agricultural productivity and inputs. The number of wives in a household does not have a significant effect on maize yield, agricultural inputs or crop value. The estimate of the effect of joint plot management with the first wife is negative for all outcomes, but it is not significant. There is no consistent or significant effect of joint management with the second wife. Likewise, the interaction of joint management with either wife and the total number of wives does not have a significant relationship with the outcomes.

Finally, I examine the effect of polygyny on agricultural productivity using a fixed effect for the household, plot and crop. The results of this estimation are shown in Table 4.6. The fixed effect term in this estimation captures the average levels of each household, plot and crop, thus the resulting coefficients show the impact of the variation over time of that household characteristic from its household-level mean. Thus, any covariate that does not change over time is dropped from the regression. Eight percent of husbands added a wife between 2009 and 2013. Sixteen percent of plots became jointly managed with a wife between 2009 and 2013.

In Table 4.6, the total number of wives has a positive and significant effect on quantity of agricultural labor. Perhaps due to the small amount of variation across the three

waves and the large amount of variation captured by the fixed effect and due to large standard errors, these estimations do not show a significant effect of changes in joint management on yield, inputs or crop value. The coefficients on the joint management effects are also not consistently positive or negative. Further, the interaction of joint management with the number of total wives does not have any significant effect on the outcomes.[8] This table does not provide evidence of allocative inefficiency in farming.

Because this fixed effects estimation includes three observations for each household, plot and crop over the time period, these estimated effects do not distinguish between actual effects of polygyny and changes in household dynamics, or agricultural production over time. For example, it is possible that the household acquired more social capital between 2009 and 2013, which contributed to higher quality labor inputs rather than the changes in wives alone affecting labor inputs.

4.6 Conclusion

Polygyny can alter the bargaining power structure within a poor agricultural household through resource competition or through cooperation and additional labor. I estimate the effect of the number of wives in a household, wife order and joint plot management on agricultural productivity for farming households in Tanzania. The results showing the effect of polygyny itself on yield do not suggest inefficiency. The total number of wives in a household has both positive and negative effects on yield, and these effects are rarely statistically significant. The effect of polygyny also does not have a clear effect on agricultural inputs or crop value. The few variables that did show some statistical significance were not consistent across years of analysis. For example the effect of polygyny on fertilizer use in 2009 is negative and significant, but the same estimation is positive and insignificant in 2013.

I expected that joint management with the first wife would be associated with lower

[8] In the combined dataset, the interaction of joint management with the second wife and total wives is 0 for 99.3% of plots. The fixed effect estimation results in perfect multicollinearity with another variable in the regression, thus the second wife interaction term is dropped from the estimate.

agricultural productivity and input allocation, as a reflection of inefficient allocation of resources to plots that are (at least partly) managed by women. These results would have been in agreement with (Udry, 1996). I also expected that joint management with the second wife would have an inverse effect and would be associated with higher yield and input allocation, as an indicator of focused efforts of the entire household on that crop. However, I was not able to confirm this pattern in the empirical results. Joint management with the first wife has a both positive and negative relationship with the outcomes. Joint management with the second wife also shows both a positive and negative relationship with the outcomes.

The policy implications of this research pertain to farming extension and education services. Most extension services related to improved cultivation practices in Tanzania are tailored to men as the main audience. However, because half of all plots in this Tanzanian sample are managed by both husbands and wives, these extension services should reach all members of the household. Improved seeds, use of fertilizer and good soil practices all positively affect agricultural outcomes, regardless of plot manager. Despite the existence of extension services in Tanzania, there remains a significant amount of crop loss and missing yields in this farming data. All adult members of the household would benefit from farming extension and education services.

The results in this study expand on the concept of intra-household bargaining power by exploring marriages with more than two members. More research is needed to better understand the effect of additional wives on agricultural productivity, perhaps in samples with a larger sample of polygynous households and more variation in wives over time. I have built on the literature in polygyny and agriculture by investigating Pareto efficiency in polygynous households in East Africa, where the culture around household hierarchy is different from that in West Africa. I have shown that, despite changes in marital structure, these farming households do not exhibit signs of inefficiency as a result of polygny and joint management.

4.7 Tables

Table 4.1: Descriptive Statistics by Marital Status

VARIABLES	Always single	Always mongamous	Got monog. married	Gained more wives	Always polyg.
Yield (kg/acre)	297.17	345.05	370.81	323.17	357.81
	(14.17)	(12.16)	(24.54)	(28.08)	(33.26)
Total wives	0	1.00	0.91	1.76	2.19
	(0.00)	(0.00)	(0.01)	(0.03)	(0.03)
Fertilizer used	0.24	0.35	0.30	0.29	0.20
	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)
Farm size	3.48	6.20	6.44	8.77	8.14
	(0.11)	(0.16)	(0.23)	(0.51)	(0.62)
Soil is good quality	0.43	0.50	0.50	0.48	0.50
	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)
Log monthly consumption	10.26	10.18	10.27	10.16	10.32
	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)
Plot is jointly managed	0.00	0.70	0.59	0.67	0.60
	(0.00)	(0.01)	(0.02)	(0.03)	(0.03)
Observations	1,108	1,824	801	323	265

Standard deviations in parentheses

Table 4.2: Panel Descriptive Statistics

VARIABLES	(1) 2009	(2) 2011	(3) 2013
Yield (kg/acre)	326.503	323.814	354.278
	(14.817)	(11.309)	(16.566)
Number of wives	0.778	0.841	0.900
	(0.019)	(0.016)	(0.017)
Fertilizer used	0.279	0.306	0.304
	(0.014)	(0.012)	(0.012)
Log farmsize	5.242	5.636	6.431
	(0.173)	(0.152)	(0.198)
Soil is good quality	0.515	0.468	0.464
	(0.015)	(0.012)	(0.013)
Log monthly household consumption	10.197	10.115	10.351
	(0.019)	(0.015)	(0.014)
Plot is jointly managed	0.395	0.519	0.523
	(0.015)	(0.012)	(0.013)
Observations	1,282	1,601	1,577

Standard deviations in parentheses

Table 4.3: Cross-sectional 2009 Effect of Polygamy on Yield and Inputs

VARIABLES	(1) Maize	(2) Fertilizer	(3) Labor	(4) Crop value
Number of wives	0.191	-0.796**	-0.258*	0.155
	(0.144)	(0.334)	(0.134)	(0.174)
Joint management with wife 1	0.030	0.156*	-0.032	0.107**
	(0.063)	(0.080)	(0.032)	(0.042)
Joint management with wife 2	-0.030	-0.966	0.462*	-0.048
	(0.265)	(0.684)	(0.274)	(0.355)
Husband's years of education	0.025***	0.093***	-0.006	0.022***
	(0.009)	(0.012)	(0.005)	(0.006)
Log monthly consumption	0.102***	0.447***	-0.040*	0.172***
	(0.036)	(0.056)	(0.023)	(0.030)
Husband and wife age difference	0.003	-0.006	-0.008***	0.004
	(0.005)	(0.006)	(0.003)	(0.003)
Husband's age	-0.008***	0.001	0.005***	-0.008***
	(0.002)	(0.003)	(0.001)	(0.002)
Seeds were purchased	-0.118	-0.033	-0.046	-0.138***
	(0.089)	(0.088)	(0.035)	(0.046)
Log farm size	-0.182***	-0.087*	-0.611***	-0.206***
	(0.050)	(0.050)	(0.017)	(0.026)
Traditional type of seed	-0.143	-1.069***	0.080	-0.425***
	(0.088)	(0.138)	(0.056)	(0.072)
Intercropped	-0.182**	-0.047	-0.256***	-0.504***
	(0.089)	(0.081)	(0.032)	(0.042)
Soil reported good	0.427**	0.315*	0.443***	0.274***
	(0.171)	(0.187)	(0.075)	(0.097)
Some of crop was lost	-0.062	-0.238***	0.127***	-0.128***
	(0.091)	(0.080)	(0.032)	(0.042)
Quantity of fertilizer	0.060***		0.012*	0.059***
	(0.013)		(0.007)	(0.009)
Quantity of labor	0.110**	0.076*		0.274***
	(0.042)	(0.041)		(0.021)
Constant	4.792***	-2.368***	5.261***	8.379***
	(0.472)	(0.740)	(0.284)	(0.386)
Observations	1,318	3,696	3,696	3,685
R-squared	0.248	0.103	0.317	0.202

Cluster-robust standard errors in parentheses *** p<0.01 ** p<0.05 *p<0.1

Table 4.4: Cross-sectional 2011 Effect of Polygamy on Yield and Inputs

VARIABLES	(1) Maize	(2) Fertilizer	(3) Labor	(4) Crop value
Number of wives	-0.625*	-0.254	-0.115	-0.083
	(0.353)	(0.441)	(0.246)	(0.439)
Joint management with wife 1	0.132	-0.240	0.034	-0.007
	(0.264)	(0.409)	(0.217)	(0.238)
Joint management with wife 2	-0.514	0.231	0.508	-1.650
	(1.800)	(1.662)	(1.161)	(1.748)
Interaction of joint mgmt w wife 1	-0.133	-0.139	0.040	-0.037
and number of wives	(0.232)	(0.333)	(0.142)	(0.193)
Interaction of joint mgmt w wife 2	0.705	0.132	-0.134	1.175**
and number of wives	(0.515)	(0.682)	(0.349)	(0.519)
Seeds were purchased	0.357*	0.117	0.034	0.443**
	(0.170)	(0.256)	(0.089)	(0.162)
Log farm size	-0.077	-0.010	-0.501***	0.034
	(0.071)	(0.095)	(0.045)	(0.047)
Intercropped	-0.119	0.345**	-0.110	-0.379***
	(0.083)	(0.136)	(0.069)	(0.104)
Soil is good quality	0.144	0.107	0.002	0.416***
	(0.145)	(0.284)	(0.169)	(0.137)
Some of crop was lost	-0.087	-0.032	-0.013	-0.046
	(0.074)	(0.249)	(0.055)	(0.074)
Log monthly consumption	0.119	0.117	0.073	0.140
	(0.095)	(0.301)	(0.055)	(0.092)
Husband and wife age diff	0.000	0.001	-0.001	0.014**
	(0.007)	(0.010)	(0.004)	(0.007)
Husband's age	-0.012**	0.001	0.006*	-0.012**
	(0.004)	(0.008)	(0.003)	(0.005)
Husband's years of education	-0.024	0.081***	-0.009	0.004
	(0.016)	(0.024)	(0.010)	(0.011)
Quantity of fertilizer	0.042		0.058***	0.028*
	(0.024)		(0.014)	(0.015)
Quantity of labor	0.235***	0.428***		0.297***
	(0.064)	(0.122)		(0.048)
Constant	3.911***	-2.013	3.559***	6.971***
	(0.947)	(2.974)	(0.693)	(0.871)
Observations	480	808	808	808
R-squared	0.288	0.237	0.441	0.295

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.5: Cross-sectional 2013 Effect of Polygamy on Yield and Inputs

VARIABLES	(1) Maize	(2) Fertilizer	(3) Labor	(4) Crop value
Number of wives	0.682	1.564	0.561	-0.150
	(0.595)	(1.459)	(1.693)	(0.722)
Joint management with wife 1	-0.086	-1.344	-0.723	-0.216
	(0.438)	(0.905)	(0.578)	(0.386)
Joint management with wife 2	-0.147	-4.086	3.088	2.198
	(2.108)	(5.474)	(6.415)	(1.695)
Interaction of joint mgmt wife 1	0.199	0.755	0.608	0.210
and number of wives	(0.299)	(0.650)	(0.469)	(0.292)
Interaction of joint mgmt wife 2	0.176	1.348	-0.754	-0.625
and number of wives	(0.900)	(1.927)	(2.448)	(0.607)
Seeds were purchased	-0.089	-0.048	0.245	0.162
	(0.206)	(0.149)	(0.274)	(0.130)
Log farm size	-0.362***	-0.149	0.043	-0.285***
	(0.112)	(0.110)	(0.143)	(0.069)
Intercropped	0.025	0.326	-0.167	-0.400***
	(0.072)	(0.282)	(0.241)	(0.109)
Soil is good quality	0.552**	-0.296	0.644	0.521**
	(0.224)	(0.654)	(0.395)	(0.209)
Some of crop was lost	-0.395**	0.280	0.325*	-0.129
	(0.186)	(0.204)	(0.181)	(0.094)
Log monthly consumption	0.246	0.658***	0.633***	0.255**
	(0.146)	(0.121)	(0.204)	(0.103)
Husband and wife age diff	-0.013	-0.018*	0.010	-0.003
	(0.009)	(0.010)	(0.019)	(0.009)
Husband's age	0.000	-0.004	0.003	-0.001
	(0.007)	(0.009)	(0.009)	(0.003)
Husband's years education	0.006	0.071***	0.082**	0.014
	(0.035)	(0.024)	(0.036)	(0.014)
Quantity of fertilizer	0.138***		-0.041	0.074***
	(0.038)		(0.081)	(0.024)
Quantity of labor	0.088*	-0.032		0.097***
	(0.047)	(0.062)		(0.021)
Constant	0.977	-6.877***	-3.573	6.376***
	(2.137)	(1.694)	(3.262)	(1.372)
Observations	459	765	765	733
R-squared	0.315	0.314	0.163	0.323

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.6: 2009-2011-2013 Panel Effect of Polygamy on Yield with Household-Crop-Plot Fixed Effect

VARIABLES	(1) Maize	(2) Fertilizer	(3) Labor	(4) Crop value
Number of wives	0.200	0.195	1.059***	0.092
	(0.247)	(0.264)	(0.285)	(0.137)
Joint management w wife 1	0.020	0.829	-0.435	-0.123
	(0.776)	(0.789)	(0.857)	(0.402)
Joint management w wife 2	0.916	0.955	-0.887	0.485
	(0.918)	(0.927)	(1.006)	(0.473)
Interaction joint mgmt wife 1	-0.103	-0.805	0.552	0.060
and number of wives	(0.771)	(0.777)	(0.843)	(0.396)
Number of wives outside household	-0.110	0.393*	0.985***	0.148
	(0.207)	(0.228)	(0.246)	(0.117)
Seed type	-0.062	0.184***	-0.152**	-0.209***
	(0.057)	(0.065)	(0.071)	(0.034)
Log farm size	-0.470***	-0.018	-0.407***	-0.265***
	(0.062)	(0.071)	(0.077)	(0.037)
Plot was intercropped	0.012	0.029	-0.084	-0.104**
	(0.074)	(0.092)	(0.100)	(0.047)
Soil is good quality	0.179	0.099	-0.081	0.082
	(0.145)	(0.160)	(0.174)	(0.082)
Crop loss	-0.131*	0.079	0.062	-0.066
	(0.075)	(0.082)	(0.089)	(0.043)
Steep slope	-0.155	-0.321	0.603**	0.046
	(0.180)	(0.219)	(0.237)	(0.113)
Soil is avg quality	0.181	0.027	-0.014	0.020
	(0.139)	(0.155)	(0.168)	(0.080)
Log monthly household consumption	0.010	0.312***	0.520***	0.122***
	(0.063)	(0.072)	(0.077)	(0.037)
Number of children	0.047	0.031	-0.023	0.010
	(0.040)	(0.045)	(0.049)	(0.024)
Quantity of fertilizer	0.054***		0.082***	0.066***
	(0.017)		(0.021)	(0.010)
Quantity of labor	-0.025	0.070***		0.078***
	(0.017)	(0.018)		(0.010)
Observations	2,249	4,125	4,125	4,002
R-squared	0.055	0.026	0.064	0.195
Number of id	832	1,558	1,558	1,521

Cluster-robust standard errors in parentheses *** p<0.01 ** p<0.05 *p<0.1

Chapter 5

Conclusion

This dissertation presents the results from three essays on the microeconomics of development in Tanzania. The results and analysis contribute to a growing body of literature about what works in economic development.

The randomized control trial in Chapter 2 shows the improved community family planning education reduces excess fertility, providing support for the effectiveness of community-based distribution of family planning services. The larger reduction in fertility for women who received the family planning information alone provides evidence of the benefit of reduced fertility. However, the improved communication and aligned preferences for the couples intervention supports policy interventions that include both husbands and wives together. The inclusion of husbands in education-focused consultations may reduce husbands' fertility preferences, which may reduce fertility in the long run, while and the exclusion of husbands in information and access-based consultations may allow women to meet immediate demand for fertility control.

Chapter 3 shows that an entrepreneurship training program for unemployed youth in Kagera has positive effects on financial literacy, confidence in employment and ability to save. Despite the challenges of non-randomized evaluation, this study builds on a large set of literature on the impacts of job training programs in the developing world. This program focuses on the intermediary employment outcomes, such as attitudes and

perceptions, which provide insights into the mechanisms of employment trajectories for youth participants.

Chapter 4 tests for allocative efficiency across plots in polygynous farming households in Tanzania. It has been shown that polygyny can alter the bargaining power structure within a poor agricultural household through resource competition or through cooperation. However, the estimates of the effect of the number of wives in a household, wife order and joint plot management on agricultural productivity do not find any evidence of inefficiency. Although this chapter explored the impact of polygyny, wife order and joint management on yield, fertilizer, labor allocations and crop value, it found no significant effects, either or negative.

The persistence of widespread poverty of Tanzania necessitates research to understand microeconomic problems and to propose possible solutions. Fertility decisions, labor market participation and agricultural productivity play important roles in the lives of individuals and families and all three are potentially subject to problems of information, lack of education and inefficiency. This dissertation explores ways to improve the lives of Tanzanians and alleviate the burden of poverty through microeconomic research on the three topics. I aim to bolster the public understanding of what works in development using empirical analysis and evaluation. Microeconomic development that emphsizes building capabilitiy and choice pays homage to the philosophy of early economists in emphasizing individual worth and welfare as the preeminent purpose of the field of economics.

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